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VOLUME 89 • NUMBER 2 • APRIL 1984

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



ERRATUM

Some recipients of the *Review* have noted that the cover of the April 1984 issue shows an eighteenth-century map of Africa, although the legend for the map printed on page two of that issue speaks of a seventeenth-century map of North and Central America. Perhaps they concluded that either the editors are not yet aware of the continental drift that separated these land masses some time ago or we are confused about whether the subject of Peter Wood's fine essay, which the April cover was intended to illustrate, is David Livingstone or Sieur de la Salle. Actually the April cover is merely further evidence of the validity of Murphy's Law.

Since December 1983 the staff of the *AHR* has been burdened not only with the task of producing four issues of the journal but also with the design, purchase, installation, and inauguration of a computer network that will enable us to word-process (the language's newest verb) the entire publication and transmit it electronically to the presses at the printing plant. When fully operational, the new system is expected to reduce printing costs significantly. Its acquisition, however, seriously hindered editorial work on the December, February, and April issues, whose publication was delayed as a consequence. In surmounting this temporary hurdle, we have also had to rely more heavily than usual on employees of the William Byrd Press. They have labored mightily in our behalf during recent months, and we are most grateful for their good-humored cooperation.

Whenever normal publishing routines are disrupted, mistakes are likely, but fortunately most are not so detectable as that on the cover of the April issue. During the final ("blue line") stage of production at the press—after the editors had completed their work—the map intended for that cover was inadvertently exchanged for one already published last October on the cover of "African History Today." When the error was discovered, a valiant effort was made to retrieve the issue from the postal service, but to no avail. The bulk mailing center had acted with uncharacteristic speed, and copies of the issue were already on their way to destinations throughout the world.

May we ask you to admire the correct map (see the reverse side) and then attach it firmly to the front cover of your copy of the April 1984 issue? We pray that this issue will not turn into a collector's item.

Otto Pflanze, Editor

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Cover illustration: Carte de l'Amérique Septentrionale et partie de la Méridionale depuis l'embouchure de la rivière St. Laurent jusqu'à l'isle de Cayenne avec les nouvelles découvertes de la rivière Mississippi ou Colbert." This large map of North and Central America, prepared under the direction of Abbé Bernou ca. 1681–82, has long been regarded as one of the most beautiful seventeenth-century maps of the New World. It incorporated recent explorations of the upper Mississippi and left the lower portion of the river blank in anticipation of La Salle's 1682 expedition down stream. For reasons that are only now becoming clear, his journey to the gulf did not properly fill this gap in European cartographic knowledge. Instead, it led to a plausible, but mistaken, interpretation of the river's course that went uncorrected for nearly two decades. The misunderstanding had a disastrous impact on La Salle's project for a Gulf Coast colony, an unsettling tendency on French policy toward New Spain, and an untold effect on the future course of American History. A copy of the map can be examined in Gabriel Marcel, *Reproductions de Cartes & de Globes Relatifs à la Découverte de l'Amérique du XVI^e au XVIII^e Siècle* (Paris, 1893), plates 7 to 10, and it is reproduced here from the French original with the permission of the Bibliothèque Nationale. See the article in this issue by Peter H. Wood, "La Salle: Discovery of a Lost Explorer."

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of modern times. Rahe will spend the next two years in the eastern Mediterranean, continuing his studies of ancient and modern politics on a fellowship from the Institute of Current World Affairs.

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PETER H. WOOD was born in St. Louis and recalls as his earliest memory a 1946 steamboat ride on the Mississippi River. He is now an associate professor of history at Duke University and a board member of the Highlander Research and Education Center in New Market, Tennessee. He is the author of *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (1974) and co-author (with Elizabeth Fenn) of *Natives and Newcomers* (1983), a brief study of early North Carolina. In 1980 he co-edited a special issue of *Southern Exposure* on sports in the South, and he recently published an essay on the artist Winslow Homer. Currently he is working on a general study of the South between 1689 and 1763 for LSU's multivolume series, *History of the South*, and his essay on La Salle, which appears in this issue, grew out of research for that book.

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The Primacy of Politics in Classical Greece

PAUL A. RAHE

Animals less honored than we, have sagacity enough to procure their food, and to find the means of their solitary pleasures; but it is reserved for man to consult, to persuade, to oppose, to kindle in the society of his fellow-creatures, and to lose the sense of his personal interest or safety, in the ardour of his friendships and his oppositions. . . . To act in the view of his fellow-creatures, to produce his mind in public, to give it all the exercise of sentiment and thought, which pertain to man as a member of society, as a friend, or an enemy, seems to be the principal calling and occupation of his nature.

—Adam Ferguson¹

AT THE TURN OF THE CENTURY the Irish-American journalist Finley Peter Dunne wrote a column of political and social satire for a Chicago newspaper. On one occasion he touched on the ancient world, attributing the following observation to his character the sage of Halsted Street Mr. Dooley:

An earlier version of this essay was delivered as the opening address at the annual meeting of the Conference for the Study of Political Thought held in New York City, March 30–31, 1983. The original draft was prepared while I was a junior fellow at the Center for Hellenic Studies in Washington. I am especially grateful to Bernard M. W. Knox for encouragement. Peter Burian, Jan Bremmer, and James W. Muller drew my attention to evidence of particular importance. J. Joel Farber, Donald Kagan, Ann R. Steiner, Jane B. Woodson, Patrick Leigh Fermor, I. F. Stone, Solomon Wank, Sheldon Wolin, W. G. G. Forrest, Joseph Alsop, and Elizabeth Fox-Genovese read the essay in its entirety and made helpful comments. I am especially indebted to Richard Hoffman, who initiated me into the mysteries of word processing, and to Joyce Sawyer, who handles interlibrary loans for the Shadok-Fackenthal Library of Franklin and Marshall College.

I have cited the standard works of modern political philosophy by the divisions and subdivisions employed by the author (that is, by book, part, chapter, section number, and paragraph where designations of this sort are provided). For fragments surviving from works of the classical period now lost, I have followed the practice now standard among classicists of citing the author's name, the fragment or line numbers, and, in parentheses following those numbers, the surname of the editor of the collection. In this fashion, I cite the following: Ernest Diehl, ed., *Anthologia Lyrica Graeca* (Leipzig, 1925); Augustus Nauck, ed., *Tragicorum Graecorum Fragmenta* (2d edn., Leipzig, 1926); Felix Jacoby, *Die Fragmente der Griechischen Historiker* (Berlin, 1926–30, and Leiden, 1950–58); Alfred Körte, *Menandri quae supersunt* (3d edn., Leipzig, 1957–59); John Maxwell Edmonds, *The Fragments of Attic Comedy* (Leiden, 1957–61); Hermann Diels and Walther Kranz, *Die Fragmente der Vorsokratiker* (10th edn., Berlin, 1960); Edgar Lobel and Denys Page, eds., *Poetarum Lesbiorum Fragmenta* (Oxford, 1963); Valentin Rose, *Aristotelis quae ferebantur librorum fragmenta collegit* (Stuttgart, 1967); Fritz R. Wehrli, *Die Schule des Aristoteles* (2d edn., Basel, 1967–69); Martin L. West, *Iambi et elegi Graeci ante Alexandrum cantati* (Oxford, 1971–72); and Bruno Snell et al., eds., *Tragicorum Graecorum Fragmenta* (Göttingen, 1971–). For the citation of inscriptions, I have used the abbreviations listed in the *Oxford Classical Dictionary*. Unless otherwise indicated, the translations in the essay are my own.

¹ Ferguson, *An Essay on the History of Civil Society* (Edinburgh, 1966), pt. 1, sect. 5, and pt. 5, sect. 3.

I know histhry isn't throe, Hinnissy, because it ain't like what I see ivry day in Halsted Sthreet. If any wan comes along with a histhry iv Greece or Rome that'll show me th' people fightin', gettin' dhrunk, makin' love, gettin' marrid, owin' th' grocery man an' bein' without hard-coal, I'll believe they was a Greece or Rome, but not before. Historyans is like doctors. They are always lookin' fr symptoms. Thos iv them that writes about their own times examines th' tongue an' feels th' pulse an' makes a wrong dygnosis. Th' other kind iv histhry is a postmortem examination. It tells ye what a counthry died iv. But I'd like to know what it lived iv.²

Mr. Dooley's complaint reflects with great precision the dilemma faced by the modern historian of antiquity and by his readers as well. Like Mr. Dooley, we are eager to know more about ancient domestic life, and not only about family quarrels, drinking bouts, love, marriage, and the never-ending struggle to make ends meet. But on these and related matters we have very little reliable information. The things Mr. Dooley could see every day on Halsted Street in Chicago are the very things the ancients took great care to hide from each other—and ultimately from us.³

The dearth of evidence regarding the private sphere does nothing to assuage our curiosity, but it may be revealing in itself. We may not be able to say what the Greek cities died of, but the relative silence of our informants regarding domestic affairs suggests that the citizens of the *pólis* lived for something that Mr. Dooley and the residents of Halsted Street would have had a great deal of trouble comprehending. Benjamin Constant hinted at the source of our difficulty when he pointed out that modern circumstances deny us many of the pleasures associated with participation in public affairs while multiplying and invigorating those derived from attentiveness to matters lying outside the political realm.⁴ As the economy has expanded, shattering the relationship between *oikonomía* and the simple management of a household and its attached estate, the polity has contracted. In a representative democracy situated on an extended territory, politics is but seldom the focus of popular concern; on an ordinary day, family matters and money making engage the passions of the ordinary citizen. If we sometimes find ourselves sharing Mr. Dooley's incomprehension and disbelief, it is because of the world that we have lost and because of all that we have gained. To recover that world and to revive some sense of the spirit that animated it requires a forgetfulness of contemporary concerns and an imaginative effort at repossession that may tax our intellectual and even our moral resources. Prudence dictates that we begin by pondering the eclipse in ancient Greece of the private sphere that Mr. Dooley considered the chief locus of human endeavor. If Xenophon is to be trusted, it was the peculiar character of

² Dunne, *Observations by Mr. Dooley* (New York, 1902), 271.

³ It should not, then, be surprising that the little reliable information we do possess concerning the ancient Greek household and economy comes to us mainly from the law courts of Athens, where citizens were forced to divulge and to discuss in broad daylight matters that, under any other circumstances, they would have preferred to keep in the dark. Were it not for the corpus of fourth-century Athenian forensic orations, students of ancient Greek domestic life would be almost entirely at a loss. If we lacked this evidence, we would find it virtually impossible to interpret the little that can be gleaned from the archaeological remains, from the Greek inscriptions, from comedy, and from the rest of Greek literature.

⁴ Constant, "De la liberté des anciens comparée à celle des modernes," in M. Édouard Laboulaye, ed., *Cours de politique constitutionnelle*, 2 (Paris, 1861): 547–48.

the distinction the Greeks drew between public and private and the relative weight they gave to each of the two spheres which set them apart from the barbarians.⁵ That understanding and that evaluation separate them from us in much the same way.

IN RECENT YEARS scholars have commonly alluded to the Greek quest for political solidarity (*homónoia*) and to the attendant hostility toward commerce by saying that the ancient economy was embedded in society.⁶ This formulation has the great virtue of drawing attention to the Greek practice of rigidly subordinating the concerns generated by the market to the larger needs of the community. This phrasing is enlightening to a degree, but it masks the true character of the *pólis*. At least in classical Greece the concerns that were primary were not social; they were political. The economy was not embedded in society; the economy and the society were both embedded in the polity. The love of money was not subordinated to the desire for social status; they were alike secondary to the quest for office, for power, and for glory.⁷ It was only in postclassical Greece that war ceased to be the chief concern of the citizen and something akin to social competition replaced political rivalry.⁸ When the Macedonians and their Roman successors destroyed the full

⁵ See Xenophon *Anabasis* 5.4.30–34. Also see Herodotus *The Histories* 1.10.3; Thucydides *Peloponnesian War* 1.6.5; and Plato *Republic* 5.452c.

⁶ The phrase originates with Karl Polanyi; see his *Primitive, Archaic, and Modern Economies* (Boston, 1971), 3–37, 78–203, 238–60, 306–34, and *The Livelihood of Man* (New York, 1977), 145–276. For bibliography and discussion, see S. C. Humphreys, *Anthropology and the Greeks* (London, 1978), 31–75. Polanyi's approach has influenced ancient historians chiefly through the work of M. I. Finley; see, especially, Finley, "Aristotle and Economic Analysis," *Past & Present*, no. 47 (1970): 3–25, *The Ancient Economy* (Berkeley and Los Angeles, 1973), 17–61, 150–76, *The World of Odysseus* (3d edn., New York, 1978), and "Land, Debt, and the Man of Property at Athens," "Technical Innovation and Economic Progress in the Ancient World," "Mycenean Palace Archives and Economic History," "Homer and Mycenae: Property and Tenure," and "Marriage, Sale, and Gift in the Homeric World," in his *Economy and Society in Ancient Greece* (London, 1981), 62–76, 176–95, 199–245. Pierre Vidal-Naquet has surveyed Finley's early work in "Économie et société dans la Grèce ancienne: L'Oeuvre de Moses I. Finley," *Archives européennes de sociologie*, 6 (1965): 111–48. Also see Michel M. Austin and Pierre Vidal-Naquet, *Economic and Social History of Ancient Greece* (Berkeley and Los Angeles, 1977). For further discussion, see notes 7, 32, and 52, below.

⁷ Polanyi's formula obscures the difference between warrior republics like those of classical Greece and subpolitical artisan communities of the sort Mack Walker described in his remarkable work, *German Home Towns* (Ithaca, N.Y., 1971). G. E. M. de Ste. Croix avoided this difficulty by emphasizing the Greek employment of slave labor; Ste. Croix, *The Class Struggle in the Ancient Greek World* (Ithaca, N.Y., 1981). M. I. Finley achieved much the same end by asserting the fundamental importance of the Greek peasantry's acquisition of political rights; *The Ancient Economy*, and *Ancient Slavery and Modern Ideology* (London, 1980), 67–149. I do not deny the importance of what these two men pointed out, but I doubt very much whether an interpretation of the *pólis* guided by Karl Marx or even Max Weber can adequately answer Mr. Dooley's question. To interpret instrumentally the fact that the Greeks placed such a high premium on the privilege of citizenship is to dismiss the Hellenic understanding of what made life worth living. See pages 273–88, below.

⁸ Even then, the kinship is distant. Early in the second century A.D., the Younger Pliny was dispatched to Bithynia-Pontus as the emperor Trajan's personal emissary. Pliny's letters to the emperor indicate that the various Greek cities of the province and their leading citizens had a propensity for bitter rivalry and for extravagant expenditure in a futile quest to outdo each other in constructing grand public edifices. See Pliny the Younger *Epistulae* 10.17a.3–4, 10.17b, 10.18, 10.23–24, 10.37–44, 10.70–71, 10.75–76, 10.81–82, 10.90–91, 10.98–99, 10.108, 10.113, 10.116–17; and A. N. Sherwin-White, *The Letters of Pliny* (Oxford, 1966), 525–46, 580–728. Trajan did not allow the formation of a *collegium* of firefighters at Nicomedia on the grounds that the cities of the province were already "disturbed by factions of this very kind." As he put it, "Where men are drawn together for a common purpose, their organizations—whatever name we give them and for whatever purpose they exist—turn into political clubs (*hetaerae*) in a short time." See Pliny the Younger *Epistulae* 10.34. Also note

independence of the political community, slowly reducing the *pólis* to little more than a unit of local administration, politics lost much of its dignity.

There is another, perhaps more telling, way to make this point. Those who speak of the economy as being embedded in society take for granted the distinction between government or state and society. This distinction, introduced by John Locke, foreshadows the world of the modern republic and is inapplicable to the ancient city. There was no Greek state.⁹ The ancient Hellenic republic was, as James Madison noted, "a pure democracy, . . . a society consisting of a small number of citizens, who assemble and administer the government in person."¹⁰ The *pólis* was, as the Greeks often remarked, the men. The Hellenes did not speak of the deeds of Athens, Corinth, and Megara. These were places, not polities. As the public inscriptions assert, the real actors were the Athenians, the Corinthians, and the Megarians.¹¹ The people wielded the power, and they constituted both state and society wrapped up in one. With only trivial exceptions, the Greek cities had no bureaucracies, no magistrates blessed with long tenure, no professional armies. It was futile to try to distinguish the governors from the governed. The *pólis* itself depended on the identity of soldier and civilian; the farmer had the right to own land solely by virtue of his status as a citizen. The differentiation of roles that the distinction between state and society presupposes simply did not exist. In principle and to a substantial degree in practice, the body of citizens was homogeneous.¹²

Just as there was no Greek state, so there was no civil society. The city was, as Aristotle argued, a political community (*koinonía*). It was a *Gemeinschaft*, not a

ibid., 10.92–93, 10.96.7. The civic orations of Pliny's contemporary Dio Chrysostom provide much the same impression of affairs in the communities of the province; Dio Chrysostom *Orationes* 38–51.

⁹ The term "state" was introduced by Machiavelli, who used *lo stato* to allude to "command over men"; see J. H. Hexter, "The Predatory Vision: Niccolo Machiavelli, *Il Principe*, and *lo stato*," in his *The Vision of Politics on the Eve of the Reformation* (New York, 1973), 150–78. It reached its full development in the political science of Thomas Hobbes, who would have agreed with Max Weber's definition of the state as that entity which "(successfully) claims the monopoly of the legitimate use of physical force within a given territory"; Weber, "Politics as a Vocation," in his *From Max Weber: Essays in Sociology* (New York, 1946), 78. The state is an abstract entity constituted by power. To the extent that it has a tangible existence, the state is indistinguishable from the arms by which that power is exerted—the police forces, the standing army, and the bureaucracy that make up the permanent government in every modern polity. The state is never synonymous with the body politic, and it is never itself a true community. This is evident enough from the manner in which it is consistently coupled with—and distinguished from—the individual, the church, and society. This distinction is crucial to understanding John Locke's *The Second Treatise of Government*, paras. 87–122, 211–43, ed. Peter Laslett in Locke, *Two Treatises of Government* (Cambridge, 1963). As Friedrich Nietzsche observed in this connection, "State is the name of the coldest of all the cold monsters. Coldly as well does it lie; and this lie creeps out of its mouth: 'I, the State, am the People.'" Nietzsche went on to suggest, it is "a Faith and a Love," not the State, that constitute a People. Nietzsche, *Also Sprach Zarathustra*, part I: "Vom neuen Götzen," ed. Karl Schlechta in Nietzsche, *Werke* (Munich, 1966). Also see Harvey C. Mansfield, Jr., "On the Impersonality of the Modern State: A Comment on Machiavelli's Use of *Stato*," *American Political Science Review*, 77 (1983): 849–57.

¹⁰ James Madison, *The Federalist*, no. 10, ed. Jacob E. Cooke in *The Federalist* (Middletown, Conn., 1961).

¹¹ Consider the material collected by Charles Forster Smith in "What Constitutes a State," *Classical Journal*, 2 (1906–07): 299–302. Also see Russell Meiggs and David Lewis, eds., *A Selection of Greek Historical Inscriptions* (Oxford, 1975).

¹² Yvon Garlan, *War in the Ancient World*, trans. Janet Lloyd (London, 1975), 86–103. Also see Xenophon *Vectigal* 2.3–4. It took a special decree of the assembly to extend to a noncitizen the privilege of owning land; J. Pecirka, *The Formula for the Grant of Enktesis in Attic Inscriptions* (Prague, 1966); and Austin and Vidal-Naquet, *Economic and Social History of Ancient Greece*, 95–99. Also see Xenophon *Vectigal* 2.3–4; and Stephen Holmes, "Aristippus In and Out of Athens," *American Political Science Review*, 73 (1979): 113–28, as well as Holmes's exchange with James H. Nichols, Jr., *ibid.*, 129–38.

Gesellschaft.¹³ The *pólis* was not a conspiracy of self-seeking individuals joined for mutual profit and protection in a temporary legal partnership that would be dissolved when it ceased to suit their interests. It was a moral community of men permanently united by a common way of life. As a human being the Greek possessed no rights against the commonwealth. As an adult male citizen he might demand and be granted certain privileges, but these were more than outweighed by his duties to the community at large. Here, as is often the case, language is the shadow of political reality. It is by no means fortuitous that the English word *idiot* is derived from the Greek term that designates those who prefer private pleasure to public endeavor. Because they were shirkers who took what the city had to offer and gave almost nothing in return, men of this stripe incurred scorn and ill will. In short, the peculiar division between a narrow public and a broad private realm characteristic of bourgeois regimes was alien to the Greek experience. The claim of the civic community was, in principle, total: only the household (*oikos*) proved capable of resisting absorption because the city depended on the preservation of one refuge of privacy for the procreation, rearing, and nourishment of its future citizens.¹⁴

This design left little room for women. Throughout antiquity the female members of the human race shared the fate of the *oikos*. Physically, they were not up to the rigors of hoplite warfare, and the biological function they performed in reproduction necessarily relegated them to the realm of procreation and child rearing (although it need not have confined them there forever). Prior to the emergence of the *pólis* and after its decline, when the household was strong and the political community relatively weak, custom and law accorded women a modicum of personal freedom and independence. But in the classical period, when the *pólis* was in full vigor, they occupied an unenviable status. Women were everywhere denied political rights. In some cities they could neither bring suit in the courts nor own property in their own names.¹⁵ While the men of the town spent their leisure hours loitering about grand public spaces within sight of marble or stone buildings

¹³ Aristotle *Politics* 1280a25–1281a4. Also see Pseudo-Demosthenes 25.16–17. For the import of the *Gesellschaft-Gemeinschaft* distinction, see Ferdinand Tönnies, *Community and Society* (London, 1955). The failure to grasp the importance of Tönnies's distinction for understanding the Greek *pólis* can lead one to attribute a confusion to Aristotle where none exists; R. G. Mulgan, *Aristotle's Political Theory* (Oxford, 1977), 13–37.

¹⁴ As Aristotle on one occasion acknowledged, the household is more natural than the *pólis* because it is prior to and more necessary than the political community. If he elsewhere denied this, it is because the household lacks self-sufficiency (*autárkeia*) and can therefore survive and do its proper work in promoting virtue only as part of a much larger unit. Compare *Nicomachean Ethics* 1162a17–29 with *Politics* 1253a18–29. The confusion caused by Aristotle's two statements is purely semantic in origin: from the perspective of efficient causation, the household holds priority; from that of final causation, the *pólis* is first. The household is a prerequisite for life, the *pólis* for the good life. The inevitable tension between this private community and the public community is the background for the dramatic action of Aeschylus's *Eumenides*, Sophocles' *Antigone*, and Aristophanes' *Clouds*. It is no accident that Aristophanes' *Ecclesiazusae* makes no mention of procreation: a city without households would be a city that paid little attention to the rearing of children. For a defense of the household, see Aristotle's critique of Plato's abolition of the household in *The Republic*; *Politics* 1261a4–1264b25.

¹⁵ The overall pattern is evident from Sarah B. Pomeroy's survey; Pomeroy, *Goddesses, Whores, Wives, and Slaves* (New York, 1975), 16–148. Also see W. K. Lacey, *The Family in Classical Greece* (London, 1968), 158–63, 167–70, 197–208, 212–16, 225–30; and David M. Schaps, *Economic Rights of Women in Ancient Greece* (Edinburgh, 1979). It was even considered bad form to mention a woman by name in court; David M. Schaps, "The Women Least Mentioned: Etiquette and Women's Names," *Classical Quarterly*, new ser., 27 (1977): 323–30.

possessed of a magnificence still striking today, their wives and daughters were virtual prisoners, kept in seclusion—all but the wealthiest locked within the squalid, dark, and damp confines of relatively primitive mud-brick houses. Though free by law, Greek women were held captive by custom.¹⁶

In a community that denied full dignity to private affairs, women had a public existence only through their offspring. A young girl of fourteen who suddenly found herself wed and thrust into the arms of a seasoned warrior more than twice her own age might find the experience disconcerting, but she dared not complain.¹⁷ In all likelihood, this was her one opportunity to achieve that for which, she was told, she had been born. If a woman married, became pregnant, survived the ordeal, and bore her husband a son, she assured for herself a position of respect within the family and the community at large. But by the same token, if she failed, whatever the reason, the life that remained hers was hardly worth living. The ancient medical writers inform us that women denied marriage or unable to conceive were unusually susceptible to “the sacred disease.” For this malady, when contracted under these circumstances, there was only one cure: marriage, pregnancy, and the birth of a child. Psychosomatic illness is not a modern discovery. Even in antiquity, the fear of dishonor was known to generate hysteria and, through it, epilepsy and other disorders.¹⁸ For the whole-hearted commitment to public life

¹⁶ Nepos *Praefatio* 6–7. For a detailed account of the seclusion of women in one such community, see J. P. Gould, “Law, Custom, and Myth: Aspects of the Social Position of Women in Classical Athens,” *Journal of Hellenic Studies*, 100 (1980): 38–59. Needless to say, the homes of the wealthy were far from squalid, dark, and damp. Even within the grander houses, however, women were still confined to the women’s quarters (*gunaikōnitis*), always the least attractive part of the home. See R. E. Wycherley, *How the Greeks Built Cities* (2d edn., New York, 1976), 175–97.

¹⁷ In Crete an entire age-class of young men customarily took brides upon induction into manhood; Ephorus *FGrH* 70 fr. 149 (Jacoby). Elsewhere, it was unusual for a man to wed before age thirty. This practice separated the generations so that a man would come into his inheritance at about the time he married; Aristotle *Politics* 1335a32–34. It also left him free from household responsibilities during the years when he was at the height of his physical powers and most useful to the city for the purposes of war. Women ordinarily reached menarche about age fourteen; Darrel W. Amundsen and Carol Jean Diers, “The Age of Menarche in Classical Greece and Rome,” *Human Biology*, 41 (1969): 125–32. No Greek writer recommended marriage at so tender an age, but the eagerness of Greek men to insure a legitimate succession by marrying a virgin made this the norm everywhere except at Sparta. See Xenophon *The Constitution of the Lacedaemonians* 1.3–4; and Plutarch *Lycurgus* 15.3. A similar pattern prevailed at Rome; compare Keith Hopkins, “The Age of Roman Girls at Marriage,” *Population Studies*, 18 (1965): 309–27, with the Greek evidence; Hesiod *Works and Days* 695–701; Solon fr. 27 (West); Xenophon *Oeconomicus* 7.5; Plato *Republic* 5.459b–460e, and *Laws* 4.721b–e, 6.772d, 6.785b (note *ibid.*, 8.833c–d); and Aristotle *Politics* 1335a7–35 (note *Historia Animalium* 544b14–27, 582a16–33, and *De Generatione Animalium* 766b29–37). This eagerness explains why an heiress became capable of acting as a vehicle for the transfer of her father’s property at Athens and on Thasos when she was age fourteen and at Gortyn when she was age twelve; see Aristotle *Constitution of Athens* 56.7; Jean Pouilloux, *Recherches sur l’histoire et les cultes de Thasos*, volume 1: *De la fondation de la cité à 196 avant J.-C.* (Paris, 1954), 371, no. 141 (Inscr. Inv. 1032) l. 22; and *ICr* IV 72 Col. XII 6–19, in Ronald F. Willetts, ed., *The Law Code of Gortyn* (Berlin, 1967), 50. The young age of women at marriage may explain why the exposure of female infants was common. As Mark Golden remarked, the propensity for men in their prime to marry adolescent girls would otherwise have resulted in a large population of unmarried women. Golden, “Demography and the Exposure of Girls at Athens,” *Phoenix*, 35 (1981): 316–31; and William V. Harris, “The Theoretical Possibility of Extensive Infanticide in the Graeco-Roman World,” *Classical Quarterly*, new ser., 32 (1982): 114–16. For the view that this practice must have been uncommon, see Donald Engels, “The Problem of Female Infanticide in the Greco-Roman World,” *Classical Philology* 75 (1980): 112–20. Though the marriage of a girl to a man twenty years her elder was common, sexual relations between the very young and the very old were a standard subject for mirth; see Aristophanes *Congresswomen* 877–1111; and Athenaeus *Deipnosophistae* 13.559f–560a.

¹⁸ Hippocrates *De Virginibus* 1 (Litré 8: 464–71). Because women were so rarely given the opportunity to speak for themselves, Hippocrates’ observations may provide the best evidence that we have for their outlook. This evidence can be dismissed as the remarks of a man, but the absence of cant in the surrounding discussion

that the laws and customs of the city sought to elicit from their husbands, brothers, and fathers, the women of Greece paid a very high price.

They were not alone. The primacy accorded political life presupposed more than one hierarchy. The *oikos* served an essential productive as well as a procreative function. Just as the city needed women for the replenishment of its steadily dwindling stock of citizens, so also it required laborers to provide for their nourishment. The visible *pólis* constituted by the male citizens rested on an invisible and politically inarticulate body of slaves condemned to labor in private so that their masters might be free to devote time and effort to speech and action in public. "As free men," Euripides observed, "we live off slaves."¹⁹

Nearly a century after Euripides' death, the comic poet Menander composed a variation on the same theme. To support his contention that "farming is work fit for a slave," he argued that it is not through labor but through "matters pertaining to war that a real man (*anēr*) must outdo his rivals (*hyperéchein*)."²⁰ Though brutal in tone, the verses of the Cretan poet Hybrias could apply to the active citizens of every Greek *pólis*:

In my great spear and my sword lies my wealth,
And in my fine shield, a screen for the body.
With these, I plow; with these, I reap; and
With these, I trample out sweet wine from the vine;
For it is with these that I am called the master of churls (*mnoía*)
And those who dare not bear the spear and the sword
And the fine shield, a screen for the body,
All fall down, grasping my knee and
Calling me master and Great King.²¹

suggests that we are dealing with behavior actually observed. For further discussion of these issues, see Marilyn B. Arthur, "Women and the Family in Ancient Greece," *Yale Review*, 71 (1982): 532–47; and Nicole Loraux, *Les Enfants d'Athènes: Idées athéniennes sur la citoyenneté et la division des sexes* (Paris, 1981). Also note Lysias 1.6.

¹⁹ Euripides fr. 1019 (Nauck²). The scholiast who cited this passage went on to remark that slaves were called *lusiponoi* because they did away with (*dialúousi*) toil (*pónos*) by their service (*therapeía*); see the scholia on Pindar *Pythian Odes* 4.71. If slaves had been given visibility, they might have become politically articulate. According to Seneca, the Roman Senate once debated the prudence of requiring the slaves of the city to wear a special uniform. The senators ultimately decided against such a policy because they feared making the slaves aware of their own numbers. If this were the case, the opponents of the measure argued, the slaves would have been prone to revolt. Seneca *De Clementia* 1.24.1. Seneca's testimony on this matter may be suspect, but his point is well taken. The same eagerness to deny slaves every semblance of public existence lies behind the insistent recommendation of the ancient political commentators that the citizens of a community select their slaves from a variety of different nations lest the servile population be united by a common culture, religion, and tongue. Plato *Laws* 6.777c–d; Aristotle *Politics* 1330a2528; Pseudo-Aristotle *Oeconomica* 1344b18; Varro *De Re Rustica* 1.17.5; Athenaeus *Deipnosophistae* 5.264f–265a. As the ancients recognized, rebellion is a political act, and successful political action presupposes the prior existence of that shared understanding of the nature of justice which constitutes the common way of life for a people. It can hardly be fortuitous that servile revolts have almost always had cultural, religious, or nationalist roots. See Joseph Vogt, "The Structure of Ancient Slave Wars," in his *Ancient Slavery and the Ideal of Man* (Oxford, 1974), 39–92; and Eugene D. Genovese, *From Rebellion to Revolution: Afro-American Slave Revolts in the Making of the Modern World* (Baton Rouge, La., 1979), 1–81. Also see pages 285–87, below.

²⁰ Menander fr. 560 (Körte³). Also note Heraclides Ponticus fr. 55 (Wehrli²).

²¹ Athenaeus *Deipnosophistae* 15.695f–696a. By Greek standards, the servile class of Crete was relatively free. Its members, apart from being denied access to the assembly, suffered a number of deprivations particularly worthy of Greek notice: as Aristotle observed, they were barred from the gymnasium and prohibited from the possession of arms; *Politics* 1264a20–22. Compare the measures taken in some politics against the poor; *ibid.*, 1297a29–35.

Virtually all members of the political class belonged to what Alexis de Tocqueville so aptly called “the aristocracy of masters.”²² Such men could do without slaves, Aristotle remarked, but only if they possessed automatons like the statue of Daedalus or the tripods of Homer’s Hephaestus, each able to perform its particular function when ordered and even to anticipate the master’s needs. The desire to be at leisure for political action explains why a world requiring no labor at all was one of the abiding fantasies of the Greek imagination.²³

Menander’s quip and Hybrias’s poem are important for more than one reason—for they capture not only the Greek situation but the Greek outlook as well. Heraclitus touched on the central issue: to support his claim that “war is the father of all and king over all,” he noted that it was combat that “made some men slaves and some men free.”²⁴ Of those condemned to labor for others, the house-born (*oikogeneis*) and the foundlings were but a small minority.²⁵ Throughout Hellas the ordinary slave was a barbarian taken in war, kidnapped by pirates, or sold by his kin; even where some form of dependent labor other than chattel slavery prevailed, the servile class was held down by force. By displaying cowardice in accepting their fate, its members became objects of scorn. “The race of slaves is ignoble (*hakón*),” one Euripidean character argues. “They observe everything from the perspective of the stomach (*gastér*).”²⁶

This claim was not peculiar to the last of the great tragedians. In the *Plutus*,

²² Tocqueville, *La Démocratie en Amérique*, 2: bk. 1, chap. 3, ed. J. P. Mayer in Tocqueville, *Oeuvres Complètes* (Paris, 1961–). Also see Michael H. Jameson, “Agriculture and Slavery in Classical Athens,” *Classical Journal*, 73 (1977–78): 122–45; Ste. Croix, *Class Struggle in the Ancient Greek World*, 505–09; M. I. Finley, “Was Greek Civilisation Based on Slave Labour?” in his *Economy and Society in Ancient Greece*, 97–115; and Yvon Garlan, *Les Esclaves en Grèce ancienne* (Paris, 1982), 68–81.

²³ Aristotle *Politics* 1253b23–38 should be read with Homer *Iliad* 18.373–81. For the statue of Daedalus, see Euripides fr. 372 (Nauck²); Plato *Meno* 97d, and *Euthyphro* 11b–c; and Aristotle *De Anima* 406b18. Also see Crates fr. 14–15 (Edmonds). It says something about the degree to which the quest for mastery over nature has supplanted the desire to participate in public life that the introduction of a myriad of labor-saving devices from the eighteenth century on has been accompanied by an increase, not a reduction, in the number of hours devoted to labor by the ordinary citizen. See E. P. Thompson, “Time, Work-Discipline, and Industrial Capitalism,” *Past & Present*, no. 38 (1967): 56–97. For the relation of work to time in premodern society, see Jacques Le Goff, *Time, Work, and Culture in the Middle Ages* (Chicago, 1980), 29–52; and Edmund S. Morgan, “The Labor Problem at Jamestown, 1607–1618,” *AHR*, 76 (1971): 595–611. Also see Aristophanes *Congress-women* 651–61, and *Plutus* 510–26.

²⁴ Heraclitus fr. 53 (Diels-Kranz¹⁰).

²⁵ There is one intriguing piece of evidence bearing on the proportion of house-born slaves within the servile population of the classical period. The surviving fragments of the Athenian inscriptions recording the sale of the property confiscated in 414 B.C. from those involved in the Herms and the Mysteries Scandals list forty-five slaves and specify the origins of thirty-five; *IG*³ 421, 426. Of these, only three were *oikogeneis*; see Meiggs and Lewis, *Selection of Greek Historical Inscriptions*, no. 79. In later times, if the Roman evidence is indicative, house-born slaves came to constitute a much larger proportion of the servile population; Ste. Croix, *Class Struggle in the Ancient Greek World*, 229–39. Unfortunately, although the manumission inscriptions from Hellenistic Delphi confirm the widespread suspicion that the *oikogeneis* (some of them, no doubt, children of their masters) were the slaves most likely to be granted the privilege of freedom, they tell us little regarding the structure of the slave population as a whole. See Keith Hopkins and P. J. Roscoe, “Between Slavery and Freedom: On Freeing Slaves at Delphi,” in Hopkins, *Conquerors and Slaves* (Cambridge, 1978), 133–71, esp. 140–41. The apparent dramatic increase over time in the proportion of manumitted slaves drawn from the ranks of the *oikogeneis* may be an illusion. The difference may lie in a change in reporting procedures. Over the same period, the number of slaves whose origin is left unspecified in the inscriptions declined precipitously. For evidence concerning the enslavement of foundlings, see William V. Harris, “Towards a Study of the Roman Slave Trade,” in J. H. d’Arms and E. C. Kopff, eds., *The Seaborne Commerce of Rome*, *Memoirs of the American Academy in Rome*, no. 36 (Rome, 1980), 117–40, esp. 123. Their existence is presupposed by the conceits of New Comedy, but otherwise they are rarely mentioned.

²⁶ Euripides fr. 49 (Nauck²). Also see Sallust *Bellum Jugurthinum* 85.41. Much the same charge could be leveled against a beggar (*ptōchós*). Consider the manner in which Homer used the *gastér* of the *ptōchós* as a symbol

Aristophanes sketched the gulf separating the outlook of the master from that of his slave in strikingly similar terms. While under the delusion that wealth is the only good thing of which no human being has ever secured a sufficiency, Chremylus reinforces his point by providing a list of desirables that a man can only too easily possess in excess. One can have a surfeit of sex, poetry, and honor, of courage and ambition, and even of high office as general, he remarks. And his slave adds in response that one can have enough and more than enough of bread, sweet meats, placenta, dried figs, barley cakes, and even lentil soup. This juxtaposition, underlined by the antiphonal form that the poet adopted, was intended to elicit a laughter bred of prior contempt: in keeping with the dictates of convention, the citizen intent on mentioning the good things in life can think only of eros, poetry, and politics while the slave in precisely the same situation ponders nothing but subsistence and the pleasures of filling his belly. The root of servility was taken to be an obsessive and degrading love of mere life.²⁷

Thus, when Plato's Socrates suggests that men educated for freedom must become accustomed to fearing slavery more than death, he is not saying anything out of the ordinary: he is merely echoing the most profound convictions of a martial and slave-owning people. More than a century before, on the eve of the battle of Plataea, the Hellenes united against Persia purportedly took an oath "not to hold life dearer than liberty (*eleutheria*)." The document recording the oath may well be a forgery, but this matters little. The point is that the oath's authenticity seemed plausible to the Greeks. Throughout Hellas, men who preferred servitude to the sacrifice of life were regarded as little better than the beasts of the field. The Euripidean character who asks whether "it is not better not to live at all than to live ignobly (*kakôs*)" spoke for his fellow nationals.²⁸ For the Greeks, courage was more than a prerequisite for citizenship. Like freedom, it was virtually a precondition for the possession of humanity itself.²⁹

THE PRESENCE OF THE WOMEN AND THE SLAVES was a permanent reminder to the citizens that privacy is privative and that a life centered on domestic concerns—on Mr. Dooley's family quarrels and his drinking bouts, on love, marriage, and the never-ending struggle to make ends meet—is a life of deprivation.³⁰ No one would query Xenophon's claim that the citizens served each other as bodyguards against

for man's enslavement to the needs of the body; *Odyssey* 6.133, 7.211–21, 15.341–45, 17.226–28, 17.286–89, 17.468–76, 17.556–59, 18.1–3, 18.52–54, 18.360–64, 18.376–80. On the process of enslavement, see M. I. Finley, "The Slave Trade in Antiquity: The Black Sea and the Danubian Regions," in his *Economy and Society in Ancient Greece*, 167–75; and Yvon Garlan, "Signification historique de la piraterie grecque," *Dialogues d'histoire ancienne*, 4 (1978): 1–16, and *Esclaves en Grèce ancienne*, 59–67.

²⁷ Aristophanes *Plutus* 188–93. Consider the slave's failure to mention meat in light of Plato's comments; *Republic* 2.372a–373d, 404b–c.

²⁸ Plato *Republic* 3.386a–387b; and Euripides fr. 596 (Nauck²). Also see Plato *Gorgias* 483a–b. And note Seneca *Epistulae* 77.15: "vita si moriendi virtus abest servitus est." For the oath, see Lycurgus 1.81. For the view that the oath was a later invention, see Christian Habicht, "Falsche Urkunden zur Geschichte Athens im Zeitalter der Perserkriege," *Hermes*, 89 (1961): 1–35.

²⁹ For the relationship linking courage, freedom, and the good life, see Thucydides *Peloponnesian War* 2.36.1, 2.43.4 (note *ibid.*, 2.63.1), 4.126.2, 5.9.1, 5.9.9. Compare what Tacitus has to say of the German tribes on this issue; *Germania* 20.2.

³⁰ This is the theme of Hannah Arendt's *The Human Condition* (Chicago, 1958), a text that deserves much more attention from students of Greek antiquity than it has received.

their own slaves, and no one would deny that the emergence of republican institutions during the archaic period was in large part occasioned by the desire of the peasantry for equal protection under the law. Indeed, more than a century and a half after the close of that age, a comic poet could still speak of “freedom of speech (*parrhēsia*)” as “the shield of poverty” and suggest that “anyone who loses this freedom has thrown away the shield of life.”³¹ But to suppose, as many scholars have, that political liberty was for the ordinary citizen of the Greek *pólis* merely or even primarily instrumental is to surrender to the very incredulity that so blinded Mr. Dooley.³²

Here, as in all things, Aristotle is a more dependable guide than Karl Marx or even Max Weber. He did not make a practice of explaining away public opinion, treating it as false consciousness, as mere ideology, or even as a product of the historical process.³³ Instead, Aristotle gave the views of ordinary men a respectful hearing and attempted to make sense of them by showing that, when considered as a systematic whole, they point beyond themselves to something both nobler and more intelligible. As a consequence, he was able to give full and coherent expression to the deepest convictions of his fellow Hellenes.³⁴

³¹ Xenophon *Hiero* 4.3; and Nicostratus fr. 29 (Edmonds). Also see Plato *Republic* 9.578d–579a; and Demosthenes 21.123–25. In the latter passage the struggle against exploitation gains dignity from being linked with the struggle for something nobler—political freedom. *Quot servi*, so goes the Roman proverb, *tot hostes*; see Festus 314L; Seneca *Epistulae* 47.5; and Macrobius *Saturae* 1.11.13. For the emergence of republican institutions, see Philip Brook Manville, “The Evolution of Athenian Citizenship: Individual and Society in the Archaic Period” (Ph.D. dissertation, Yale University, 1979). The best general discussion of the archaic period is Oswyn Murray’s *Early Greece* (Atlantic Highlands, N.J., 1980). Also see Antony Andrewes, *The Greek Tyrants* (London, 1956); and W. G. G. Forrest, *The Emergence of Greek Democracy* (London, 1966).

³² See M. I. Finley, “The Freedom of the Citizen in the Greek World,” in his *Economy and Society in Ancient Greece*, 77–94, and *Politics in the Ancient World* (Cambridge, 1983), 39–49, 97–121, 134. These two works represent the mature scholarship of the ablest and most respected of the sociologically oriented historians of antiquity. As such, they exhibit admirably the virtues, the limitations, and the ultimate inadequacy of the sociological approach. The disciples of Weber and Marx, in keeping with the tendency of social-science and modern political practice since Hobbes, systematically depreciate the love of one’s own and the desire for honor as human motives. As a consequence, they neglect the central importance of civil religion, national culture, and public opinion in general and base their work on a distinction between materialism and idealism that cannot be sustained. See Finley, “The Ancient City from Fustel de Coulanges to Max Weber and Beyond,” in his *Economy and Society in Ancient Greece*, 3–23, and *Ancient Slavery and Modern Ideology*, 11–66. If J. K. Davies were correct in supposing that “the fundamental vectors of Athenian society” were “the needs for food, shelter, security, and conflict resolution,” then not much more than the accident of circumstance would distinguish the ancient citizen from the modern bourgeois; Davies, *Wealth and the Power of Wealth in Classical Athens* (New York, 1981), vii. In the final analysis, materialism provides no foundation for making sense out of the Greek hostility to marketplace concerns that Finley himself has done so much to unearth; “Land, Debt, and the Man of Property at Athens,” 62–76; and *Studies in Land and Credit in Ancient Athens* (New Brunswick, N.J., 1952). To understand why the Greeks had contempt for men of mercenary disposition and an aversion to the uninhibited quest for lucre, one must pay attention to what Aristotle meant when he spoke of man as a political animal. By the same token, to understand the roots of the political apathy typical of modern republics, one must pay attention to the manner in which the modern preference for limited, representative government is grounded in a radically new understanding of the purposes for which constitutional government is established. Here again, the sociological approach is enlightening—but only to a degree. See Finley, *Democracy Ancient and Modern* (New Brunswick, N.J., 1973). And consider Madison, *The Federalist*, no. 10, in light of the argument advanced by Harvey C. Mansfield, Jr.; see Mansfield, “Party Government and the Settlement of 1688,” *American Political Science Review*, 58 (1964): 933–46, “Modern and Medieval Representation,” in J. R. Pennock and G. Chapman, eds., *Representation: Nomos*, 11 (1968): 55–82, and “Hobbes and the Science of Indirect Government,” *American Political Science Review*, 65 (1971): 97–110. Christian Meier’s work on the Greek understanding of freedom is a useful corrective to the one-dimensional focus on “material benefits” and “material relations” in much recent scholarship; Meier, “Die politische Identität der Griechen,” in Odo Marquand and Karlheinz Stierle, eds., *Identität* (Munich, 1979), 371–406; and the essays collected in *Die Entstehung des Politischen bei den Griechen* (Frankfurt am Main, 1980).

³³ For the approach that Aristotle rejected, see Ste. Croix, *The Class Struggle in the Ancient Greek World*, 409–52.

³⁴ Consider Aristotle’s discussion of what he called *endoxa* (“reputable opinions”); compare *Topics* 100b21–23,

Aristotle was quite prepared to acknowledge that the political community owed its existence to private concerns—to the need for common defense against attack and to the desire for economic cooperation. But he was unwilling to suppose that such an account of the origins of the city could explain its true nature.³⁵ If the desire for mere life brought the *pólis* into being, he observed, the desire to live nobly and well sustains it. Thus, when Aristotle described man as being by nature a political animal, he was not simply asserting that human beings are gregarious. Such a claim fails to distinguish mankind from the ants and the bees. He contended instead that only in the *pólis* could gregarious beings of this sort “have a share in the good life.”

To understand what Aristotle meant by the good life, we must take careful note of those faculties that distinguish man from the beasts. In Aristotle's view, human beings are set apart from the other animals not by their capacity for self-expression but rather by their capacity for rational speech (*lógos*). Man possesses more than mere voice (*phōnē*); he can do more than just intimate that he feels pleasure or pain. Thus, his humanity is in no way constituted by his ability to speak out, to get a load off his chest, to give vent to his spleen. He could just as well purge his emotion by the inarticulate utterance of the beast. For Aristotle *lógos* is something more refined than the capacity to make private feelings public: it enables the human being to perform as no other animal can; it makes it possible for him to perceive and make clear to others through reasoned discourse the difference between what is advantageous and what is harmful, between what is good and what is evil, and between what is just and what is unjust. The sharing of these things, Aristotle insisted, constitutes the household and the *pólis* each as a community (*koinonía*).

Aristotle could therefore assert that someone who by nature belongs outside the political community must be either a god or a hunted animal (*thērion*)—alone and at war with the world. It matters little whether the individual lives in solitude, in slavery, as a metic, or under the rule of a tyrant or king. Human beings, other than philosophers, are rendered servile and virtually subhuman by the circumstances or

with *Nicomachean Ethics* 1098b9–12, 1098b27–31, 1145b2–7, 1153b25–28, *Eudemian Ethics* 1216b26–1217a18, 1235b13–18, *Politics* 1280a9–25, 1281a42–b38, and *Rhetoric* 1355a14–18, 1361a25–27, 1398b20–1399a6, 1400a5–14. Aristotle's principle is in keeping with Socrates' practice in the Platonic dialogues. By the same token, the task of political science as understood by Aristotle is identical to that of science in general as understood by Plato and his followers: both aimed at making sense of the ordinary man's perceptions; both sought in this way “to save *tà phainómena*.” Compare Aristotle *Eudemian Ethics* 1216b26–1217a18, 1235b13–18, and *Nicomachean Ethics* 1145b2–7, with the famous claim of Eudemus, as recorded in Simplicius *De Caelo* 488.18–24 (Heiberg). See Harold Cherniss, “The Philosophical Economy of the Theory of Ideas,” *American Journal of Philology*, 57 (1936): 445–56; G. E. L. Owen, “*Tithenai ta phainómena*,” in S. Mansion, ed., *Aristote et les problèmes de méthode* (Louvain, 1961), 83–103; and Martha Craven Nussbaum, “Saving Aristotle's Appearances,” in Malcolm Schofield and Nussbaum, eds., *Language and Logos: Studies in Ancient Greek Philosophy Presented to G. E. L. Owen* (Cambridge, 1982), 267–93. In contrast, the modern political science of Thomas Hobbes and his successors, like the modern physical science of Descartes and his admirers, aims at the achievement of absolute certainty and begins with the categorical rejection of “those opinions which are already vulgarly received.” Hobbes, *The Elements of Law*, bk.1, chap. 13, para. 3, ed. Sir William Molesworth in *The English Works of Thomas Hobbes*, 4 (London, 1840): 71–73; and Descartes, *The Discourse on Method*, trans. Elizabeth S. Haldane and G. R. T. Ross in *The Philosophical Works of Descartes*, 1 (Cambridge, 1969): 81–130. For an elegant example of historical inquiry pursued in accord with Aristotelian principles, see Claude Nicolet, *The World of the Citizen in Republican Rome* (Berkeley and Los Angeles, 1980).

³⁵ In general, Aristotle was inclined to suppose that the end (*télos*) rather than the origin of an entity is determinative. As he put it, “what is posterior in the order of becoming is prior in the order of nature”; compare *Physics* 261a13–15 with *ibid.*, 260b18–19.

conscious choices that deny them participation in the political life. They are rendered servile and virtually subhuman because they are prevented from developing fully those faculties of rational argument (*lógos*) and cooperative action (*práxis*) that men possess and that other nonpolitical animals lack. We exclude slaves from the political community, Aristotle explained, because some men by nature lack the capacity for prudential deliberation (*tò bouleutikón*) regarding the advantageous, the good, and the just; we exclude women even though they possess this capacity because it is without authority (*ákuros*) over them; and we exclude children because they possess it in incomplete form. For all but the handful of men capable of the quasi-divine existence devoted to *theōría*, the fully human life is a life of *práxis* conducted in accord with the dictates of *lógos*.³⁶

Aristotle was not the only Greek writer to suppose that man is fully human only when engrossed in public deliberation. A quarter of a millenium prior to the appearance of *The Politics*, Alcaeus betrayed much the same outlook. In his song of exile the poet mourned the loss of his native Mytilene, but he did not dwell on the pleasures of the hearth and the comforts of domestic life. He intoned instead a dirge with a political theme. "What a wretch I am," he wrote,

Condemned by fate to live the life of a country boor.
I yearn, Agesilaidas, to hear the herald summon the assembly
And the council. These things that my father and my father's father
Grew old possessing among the citizens (who do each other harm)—
From these I am cast out: an exile on the frontiers. Like Onomades,
I have made my home in solitude here, and I [plot] war as one
In whose veins flows the blood of the wolf (*lukaimíais*).³⁷

There was evidently nothing novel in Aristotle's conviction that men destined by nature to live outside the *pólis* were, like Homer's Cyclops, little better than beasts of prey. Alcaeus even suggested that fortune can act in nature's place and make a hunted animal of a man.

The Mytilenian poet was not alone in his understanding of exile. Euripides

³⁶ Aristotle *Politics* 1252b27–1253a39. Also see *ibid.*, 1280a25–1281a10, and *Nichomachean Ethics* 1097a15–1098b8, 1169b16–18. Aristotle used the phrase *politikón zōon* elsewhere in a less precise sense to group human beings with bees, wasps, ants, and cranes; *Historia Animalium* 487b33–488a13. For his belief in the ultimate superiority of the philosophical life, see *Nichomachean Ethics* 1177a12–1178a8 (note *ibid.*, 1142a23–30, and *Metaphysics* 982b1–983a20), and *Politics* 1324a5–1325b32 (note *ibid.*, 1333b29–35). Also see John M. Cooper, *Reason and Human Good in Aristotle* (Cambridge, Mass., 1975); and Carnes Lord, "Politics and Philosophy in Aristotle's *Politics*," *Hermes*, 106 (1978): 336–57. For a somewhat different view, see Marcel Detienne, *Dionysos Slain*, trans. Mireille Muellner and Leonard Muellner (Baltimore, 1979), 53–67. Note Cicero *De Officiis* 1.16.50–17.58. For a discussion of the vital distinction between political or prudential speech and self-expression, see Harvey C. Mansfield, Jr., *The Spirit of Liberalism* (Cambridge, Mass., 1978), viii–ix, 16–27, 52–71. In this context, it is perhaps worth noting that Hobbesian man is modeled on Aristotle's inarticulate, hunted animal; consider Thomas Hobbes, *Leviathan*, ed. C. B. Macpherson (Harmondsworth, 1968), chap. 13. Also see Leo Strauss, *Natural Right and History* (Chicago, 1953), 172–74, and *What Is Political Philosophy?* (New York, 1959), 176 n. 2. Hobbes's decision may explain why, in the liberal democracies of the twentieth century, freedom of expression has silently replaced freedom of speech. Where the latter is understood solely as an instrument and where it comes to be taken for granted, the concern for its preservation can easily be supplanted by a longing for the former.

³⁷ Alcaeus fr. 130.16–25 (Lobel-Page). In translating the term *lukaimíais* and in filling the lacuna in the papyrus, I have followed the lead of C. M. Bowra's *Greek Lyric Poetry* (2d edn., Oxford, 1961), 145–47. For a different interpretation of the last two lines, see Denys Page, *Sappho and Alcaeus* (New York, 1959), 198–209, esp. 205–06.

touches on the same issue in *The Phoenician Women* and dealt with it in a strikingly similar way. When Jocasta asks her banished son Polyneices whether “being deprived of one’s fatherland is really a great evil,” the young man responds that it is “the greatest of evils—worse when experienced than when depicted in speech.” When urged to explain in detail what he means, Polyneices does not at first mention physical insecurity, so often the constant companion of a man without a country. He notes instead that “the most annoying aspect” of his plight is that the exile lacks “freedom of speech (*parrhēsia*).” To be sure, when pressed by his mother, Polyneices does allude to the hardships he faced in the days immediately following his departure from Thebes. Like Aristotle, he is more than willing to grant the desire for survival its due. But even when confronted with maternal concern he makes only passing reference to what James Madison called “the great principle of self-preservation.” Death by violence is not for men of Polyneices’ mettle the *summum malum* that it was for Thomas Hobbes and those within the liberal tradition of political thought that he inspired. This Theban exile nowhere treats citizenship and the political rights that go with it—in the fashion of Locke, Montesquieu, and the American Founding Fathers—as simply, or even chiefly, an instrument for the protection of man’s inalienable right to life, liberty, and property. He treats political freedom instead as an end in itself.

As the son-in-law of Argos’s king, Polyneices is in secure possession of life and the means to sustain it in comfort. What he lacks is that “freedom of speech” which belongs solely to men of independent means participant in politics. Thus, when Jocasta compares the exile’s fate to that “of a slave unable to say what he thinks,” her son signals his assent by remarking that a man without a country “must bear with the brute ignorance of those who wield power.” This is “a painful thing,” Jocasta responds, “to share in the folly (*sunasopheîn*) of fools.” In such circumstances, Polyneices sadly acknowledges, “one must be servile for the sake of advantage (*kérdos*)—and this contrary to nature (*parà phúsin*).” Even when freed from material want, the exile is no better off than the merchant or slave. All three lack the dignity of public existence, and all three remain trapped within the narrow world of the household: for they are forced by human necessity—by the fear of violence and death, or by the love of money and the sense of security it brings—to pursue *kérdos* at the expense of all honor.³⁸

The value that Alcaeus, Polyneices, and men like them accorded freedom of speech and the weight they gave to political participation help explain why the Greeks harbored such contempt for the barbarians. Even before the appearance of the Medea, when Hellas was but a scattering of obscure settlements hidden in the shadow of distant Assyria, Phocylides could write,

³⁸ Euripides *Phoenissae* 385–442; Madison, *The Federalist*, no. 43; and Hobbes, *The Leviathan*, chap. 13. Also see Euripides fr. 313 (Nauck²). Note the passage in which an exiled Plataean articulates what it means to have one’s *pólis* destroyed. “The loss of our common life,” he remarked, “causes each of us to have private hopes only. I think that you are not ignorant of the other sorts of *shame* which poverty and exile engender.” Isocrates 14.49–50. The Greek word *parrhēsia* makes its first appearance in the works of Euripides and Democritus; Euripides *Hippolytus* 422, and *Ion* 672, 675; and Democritus fr. 226 (Diels-Kranz¹⁰). For a thorough survey of the Athenian evidence, see Kurt Raaflaub, “Des freien Bürgers Recht der freien Rede,” *Studien zur antiken Sozialgeschichte: Festschrift Friedrich Vittinghoff*, ed. Werner Eck et al. (Cologne, 1980), 7–57.

A small *pólis* on a headland
Is superior to senseless Nineveh
If its affairs are conducted
In an orderly manner.³⁹

After the Greeks had exhibited their prowess against the conquerors of all Asia, they could not be stopped. Aeschylus was not the first—nor was he the last—to suggest that men who submitted to the rule of a king were slaves.⁴⁰ Herodotus picked up the refrain—and in Aristotle's day the great minds of the age were disputing whether the subjects of the Great King were obsequious by nature, by education, or because of the climate.⁴¹ This attitude touched even the king's court. Persia's monarch governed his vast empire as a household; and, when the king spoke of *manā badaka* ("my subject or vassal"), his Greek scribe—lacking a vocabulary for feudal relations—employed the term *doûlos*. The great magnates of the royal house were not women; they were not children: they could only be slaves.⁴²

Much the same outlook colored the Greek view of poverty. What the Greeks feared most from penury was not the discomfort but the indignity, not the lack of security but the loss of independence. When Plato equated "being poor" with "lacking power over oneself" and when Demosthenes wrote that "destitution (*penia*) compels free men to do many things that are slavish and base," they were restating a theme announced by Theognis centuries before. "To die, my dear Cynus," he wrote, "is better for a poor man than to live worn out by an indigence hard to bear."

Poverty does more to bring a brave (*agathós*) man under the yoke
Than anything else—more, Cynus, than grizzled age and the ague.
To flee poverty, such a man must hurl himself from the high rocks
Into the vastness of the sea. For poverty will subdue any man,
And he will be unable to say or do anything of note:
For his tongue will be tied.⁴³

Destitution was for the poor man what exile had been for Alcaeus and for Polyneices. Because these rendered a man subservient to the wealthy and the powerful men, they robbed him of his capacity for great deeds and political speech. Cicero—in paraphrasing a treatise by the Greek philosopher Panaetius of Rhodes—summed up the situation brilliantly when he wrote that "the workman's

³⁹ Phocylides fr. 4 (Diehl).

⁴⁰ I see no reason to doubt the authenticity of the inscription recording Darius's letter to the satrap Gadatas; see Meiggs and Lewis, *Greek Historical Inscriptions*, no. 12.4

⁴¹ Compare Aeschylus *Persians* 50, 74–75, 234, 241–42, 402–04, 584–97; Herodotus *The Histories* 7.101–04, 7.135–36; and Euripides *Helen* 276, and *Iphigenia in Aulis* 1400–01; with Isocrates 4.131–32, 4.150–52, 4.181–82, 5.107, 5.120–24, and *Epistulae* 3.5, 9.19; Plato *Republic* 4.435c–436a; Aristotle *Politics* 1285a15–29, 1313a34–b10, 1327b23–36. Also see Hippocrates *Airs, Waters, and Places* 12, 16; and Cicero *Republic* 1.33.50.

⁴² Compare Roland Kent, *Old Persian* (New Haven, 1950), nos. DB I.19, II.19–20, II.29–30, II.49–50, II.82, III.13, III.31, III.56, III.84–85, V.8; with Meiggs and Lewis, *Greek Historical Inscriptions*, no. 12.4. Also see Geo Widengren, *Der Feudalismus im alten Iran* (Cologne, 1969), 12–21, 32–34, 38. For the Persian empire as an *oikonomia éthnous*, see Aristotle *Politics* 1285b29–33.

⁴³ Plato *Epistulae* 7.351a; Demosthenes 57.45; and Theognis 173–78, 181–82 (West). Also see Xenophon *Hellenica* 2.3.47–49; and Isocrates 14.50. Also note Homer's treatment of the plight of the beggar; see note 26, above.

wage is itself the pledge of his servitude.”⁴⁴ In Greece a proud man facing old age in straitened circumstances was able to choose day labor over begging, but, for the sake of his freedom, he was expected to sacrifice every prospect of receiving support when weak and no longer fit for work and to prefer the instability of the labor exchange to dependency bred of prolonged employment in the service of another.⁴⁵ To be brief: the ancient hierarchy is the reverse of the modern. The Greeks did not value political freedom for the sake of life, liberty, and property; they valued the last three for the sake of the first.

This, more than anything else, explains Mr. Dooley’s confusion: for the opposition of priorities is the root cause of many of the differences that distinguish the world of Halsted Street from that of ancient times. The founders of the liberal tradition explicitly rejected Aristotle’s conviction that the human being is a political animal. They took their bearings not from man’s capacity for public deliberation and cooperative action but rather from his fear of death and aversion to pain. As a consequence, they considered the Greeks’ set of preferences to be a travesty of common sense. No one stated this more clearly than Montesquieu. The Hellenes were guilty, he contended, of having “confounded the power of the people with the liberty of the people.” They established republics that were “not in their nature free states.” Had they understood political freedom in light of its true purpose, they would have recognized that it is not first and foremost a sharing in power but that it “consists in security, or at least in the opinion that one has of one’s security.” In Montesquieu’s view the Greeks placed far too much emphasis on public spiritedness and rendered the citizens of their polities insecure by subjecting them to the purview of their fellows. “Virtue itself has a need for limits,” he argued. True “political liberty is to be found only in moderate governments” that leave the citizens to themselves. Not Sparta but Great Britain and, ultimately, the United States of America—these were to be the models for the constitution of liberty in modern times.⁴⁶

Although neither Mr. Dooley nor many of the other residents of Halsted Street would have had much use for a theory so abstract, they would certainly have been in agreement with the spirit of Montesquieu’s observations. The citizens of liberal republics are left pretty much to their own devices. They live in commercial and

⁴⁴ Cicero *De Officiis* 1.42.150. Also see Isocrates 14.48. Cicero claimed to have followed Panaetius closely, not simply translating Panaetius’s book on moral obligation but introducing only a few alterations; *De Officiis* 2.17.60, 3.2.7; and *Epistulae ad Atticum* 16.11.4. See P. A. Brunt, “Aspects of the Social Thought of Dio Chrysostom and the Stoics,” *Proceedings of the Cambridge Philological Society*, 199 (1973): 9–34, esp. 26–34.

⁴⁵ When Socrates’ old comrade Eutherus equates working as another’s *epitropos* with slavery, he does so on the basis of an appeal to public opinion: Xenophon *Memorabilia* 2.8.1–6. For further discussion, see Claude Mossé, “Les Salariés à Athènes au IV^e siècle,” *Dialogues d’histoire ancienne*, 3 (1976): 97–101. Throughout antiquity, personal service was equated with servitude; see Lucian *De Mercede conductis potentium familiaribus* 7–9, 23–25. Note Ulpian’s allusion to “those who occupy the place of slaves (*eos, quos loco servorum habemus*)” and must therefore be counted as members of the household (*familia*); *Digest* 43.16.1.16–19. One consequence was that full citizens could rarely, if ever, be found to manage a rich man’s farm; Rhona Beare, “Were Bailiffs Ever Free Born?” *Classical Quarterly*, new ser., 28 (1978): 398–401; and Gert Audring, “Über den Gutsverwalter (*epitropos*) in der attischen Landwirtschaft des 5. und des 4. Jh. v. u. Z.,” *Klio*, 55 (1973): 109–16. Where a monarchy had supplanted a popular regime and a single household (*res privata*) had absorbed the political realm (*res publica*), painful adjustments had to be made by those who coveted power and fame; Lucian *Apologia* 11–12; and Tacitus *Germania* 25.3.

⁴⁶ Montesquieu, *L’Esprit des lois*, bk. 11, chaps. 2–5, and bk. 12, chaps. 1–2.

technological societies that multiply the possibilities of private enjoyment. They possess freedom of speech, but the very size of the polities in which they reside generally robs that speech of consequence. As a result, the citizens develop a taste for domesticity. They are quick to resent any invasion of the broad realm of privacy that the regime guarantees them, and they often express the fear, even on the slightest of pretexts, that there will be a wholesale abrogation of their rights as individuals. So long as there are free elections, this fear may lack substance. But, as both Montesquieu and Tocqueville had occasion to note, it is the very anxiety of the citizens that promotes the vigilance which in turn obviates the danger. Even in time of war, the liberal democracies have shown remarkable respect for the privacy of the ordinary citizen.⁴⁷

The founders of the liberal tradition foresaw trouble, but not from this quarter. They worried instead that the citizenry would eventually lose all semblance of public vigilance. Adam Ferguson sounded the warning on the eve of the American Revolution. Perhaps because he was a Gaelic-speaker reared among the clans in the wild highlands of eighteenth-century Scotland, perhaps because he had passed nearly a decade in service as chaplain to the Black Watch, Ferguson was more acutely aware than his friends and colleagues David Hume and Adam Smith that the emergence of commercial society would inevitably be accompanied by a decline in the martial fervor that was the ultimate guarantor of political freedom. Ferguson feared "that remissness of spirit, that weakness of soul, that state of national debility, which is likely to end in political slavery." "Every successive art, by which the individual is taught to improve on his fortune, is, in reality," he observed, "an addition to his private engagements, and a new avocation of his mind from the public."

If to any people it be the avowed object of policy, in all its internal refinements, to secure the person and property of the subject, without any regard to his political character, the constitution indeed may be free, but its members may likewise become unworthy of the freedom they possess, and unfit to preserve it. . . . If the pretensions to equal justice and freedom should terminate in rendering every class equally servile and mercenary, we make a nation of helots, and have no free citizens.⁴⁸

Benjamin Constant made the same point. "The danger is that we will be so absorbed in the enjoyment of our private independence and in the pursuit of our particular interests that we will renounce too easily our right of participation in

⁴⁷ *Ibid.*, bk. 19, chap. 27; and Tocqueville, *Démocratie en Amérique*, 2: bk. 2, chap. 14, and bk. 4, chap. 1.

⁴⁸ Ferguson, *Essay on the History of Civil Society*, pt. 4, sect. 2, pt. 5, sect. 3, and pt. 6, sect. 4. The gap separating Ferguson's outlook from that of his friends is reflected in Hume's finding "almost everything" in the *Essay on the History of Civil Society* so "exceptionable" that he was willing to "concur in any Method to prevent or retard the Publication." Regarding Ferguson's reflections, Hume confided in a letter to a mutual friend, "I do not think them fit to be given to the Public, neither on account of the Style nor the Reasoning; the Form nor the Matter." Hume to the Reverend Hugh Blair, February 11, 1766, in J. Y. T. Greig, ed., *The Letters of David Hume*, 2 (Oxford, 1932): 12. The work's success surprised Hume and caused him to reconsider, but not to alter, his judgment; Hume to Blair, April 1, 1767, *ibid.*, 133. Apparently, Ferguson was too sympathetic to the rude and unpolished nations of ancient and not-so-ancient times to please a man of Hume's tastes. The latter reported with evident satisfaction the view of one reader that the style of the Highlander's essay (and, one must suspect,

political power.”⁴⁹ In modern times the household and the skein of economic and social relations to which it gives support threaten to eclipse the polity altogether.

This danger existed also for the Greeks, but it was not nearly as great. Throughout the classical period, they were in accord with the judgment rendered two millennia later by Giambattista Vico. After reading and rereading the powerful opening paragraphs of Tacitus’s *Histories* and *Annals*, this great Neapolitan philosopher and student of rhetoric paused to consider the politics of his own day and observed that, when “the majority of the citizens no longer concern themselves with the public welfare, . . . the citizens have become aliens in their own nations.” For those who observe everything from the perspective of the stomach, for human beings interested chiefly in what Thomas Hobbes once called “commodious living,” the establishment of great states on extended territories and the other circumstances that inhibit day-by-day participation in political affairs on the highest levels are matters of little concern.⁵⁰ But for men like Alcaeus, Polyneices, and Theognis, to live under such conditions would have been the most bitter of fates. For them, the fatherland was more than a place of repose; it was an all-encompassing and all-absorbing way of life.

HOW THIS COULD BE THE CASE remains difficult to see. In a nation that concedes primacy to the concerns generated by the marketplace, in a polity whose citizens are dedicated to the quest for “over-all security,” politics becomes “a field of work for punier heads.” Friedrich Nietzsche made this observation, and most of us today tend to agree. In liberal democracies, politicians—particularly on the local level—inspire little respect. There is something dreary, even sordid about much of the business done by governments. The awarding of contracts and the apportionment of jobs, the leasing of public lands and the purchase of equipment, the raising of taxes and the distribution of subsidies—these and similar mundane matters are the stuff and substance of contemporary political life.⁵¹ Their predominance reflects the degree to which administration has supplanted deliberation as the chief function of public officials. It should not be surprising that men reared in such an environment have some trouble understanding the extraordinary appeal exercised in antiquity by political life.

In *The Suppliants*, Euripides gave us an inkling of what it was that aroused the Greeks’ ardor. There, he staged a debate over the relative merits of democracy and tyranny between Theseus, the leader of the “free city” of Athens, and the herald

its substance as well) savored “of the country”—not just “somewhat,” but in fact “a great deal.” Hardly anyone but a Scotsman, this worthy had remarked, “could write such a style.” Hume to William Robertson, March 19, 1767, *ibid.*, 132.

⁴⁹ Constant, “De la Liberté des anciens comparée à celle des modernes,” 558.

⁵⁰ Vico, *The New Science of Giambattista Vico* (2d edn.), trans. Thomas Goddard Bergin and Max Harold Fisch (Ithaca, N.Y., 1968), para. 1008 (Vico cited Tacitus *Annales* 1.4; and *Historiae* 1.1); and Hobbes, *The Leviathan*, chap. 13.

⁵¹ Nietzsche, *Morgenröte*, bk. 3, para. 179. In this connection, one might consider James Bryce’s celebrated discussion of the question, “why great men are not chosen Presidents.” See his *The American Commonwealth*, 1 (2d edn., London, 1891): 73–80.

dispatched by Creon, the monarch of Thebes. In attacking one-man rule, Theseus sounds all the familiar themes: the citizens' need for equal protection under the law, the absence of safeguards in a monarchy, and the penchant of tyrants for the murder of the able and hot-blooded young, for the pillaging of those with property, and for the abduction and rape of young virgins. The tone of his discourse verges on the melodramatic, but there is nothing here that the judicious Montesquieu would not have endorsed. Nonetheless, when Theseus comes to speak of political freedom itself (*tò eleútheron*), he gives it a definition that goes well beyond the simple guarantee of the safety and well-being of the citizenry. "Being free is this," he argues: "Whoever wishes to bring useful advice before the public (*es méson*) may do so. In this way, whoever longs for eminence can shine (*lamprós esti*)—while the man lacking this desire remains silent. What could be more equitable in a city than this?" The Greek *pólis* offered its citizens more than mere protection. It provided them, in addition, with a middle ground (*tò méson*) in which to display those qualities that distinguished them from animals.⁵² "Learn the political art well," Democritus advises his fellow Hellenes, "for it is the greatest of the arts; and pursue its toils—from which human beings secure greatness and brilliance (*tà lamprá*)."⁵³

To be great and to be brilliant, to shine, to be *lamprós*: this was the desire that animated the Greek *pólis*.⁵⁴ Long before Euripides and Democritus voiced the sentiments of their contemporaries, and even longer before Aristotle fully articulated the meaning of citizenship, Homer depicted a luminous world in which men shared in the nature of the gods and sought to shed mortality. Plato called this poet "the education (*paideusis*) of Hellas," and so he was: the spirit Homer propagated later gave content to a political freedom that he might himself have been unable to imagine. For the development of this understanding of freedom, the Greek conception of the divine opened up the way. Here Homer and his disciples made the decisive contribution. In one passage Herodotus reported that Homer and Hesiod were the poets responsible for giving the Greek gods their titles, their honors, their functions, and even their looks (*eídea*); in another he hinted that the paramount difference between the religion of the Greeks and that of their

⁵² Euripides *Suppliants* 438–41; also see, more generally, *ibid.*, 399–462. Euripides' choice of metaphor is not fortuitous. If the body is the ground of privacy, speech is the middle ground of publicity. See Theognis 495; Solon fr. 10.2 (West); Herodotus *The Histories* 1.206.3, 3.80.2, 3.83.1, 4.97.5, 6.129.2, 6.130.1, 7.8.82, 8.74.2; and Demosthenes 18.139. That is why *tò méson* came to be identified with the political community itself; compare Herodotus *The Histories* 3.142.3, 4.161.3, 7.164.1; with Archilochus fr. 91.30; and Theognis 678 (West). Also compare Homer *Iliad* 23.574; with Herodotus *The Histories* 3.83.3. Also note *ibid.*, 4.118.2, 8.22.2, 8.73.3. Compare Herodotus's use of *tò koinón* and its cognates: *The Histories* 1.67.5, 5.85.1, 5.109.3, 6.14.3, 8.135.2, 9.117, with 3.82.3–4, 3.84.2, with 3.156.2, 5.109.3, and with 6.50.2, 9.87.2. It is characteristic of Finley's Hobbesian reduction of ancient politics to the pursuit of "material benefits" that he managed to cite Theseus's reply to the herald and discuss at length the import of his speech while ignoring altogether what the Athenian leader has to say about eminence and the desire to be *lamprós*; Finley, *Politics in the Ancient World*, 136–39.

⁵³ Democritus fr. 157 (Diels-Kranz¹⁰).

⁵⁴ To see what is implicit in the insistent Greek use of *lamprós* and its cognates, see Herodotus *The Histories* 1.30; and Thucydides *Peloponnesian War* 2.64. For the root meaning of the term, see Herodotus *The Histories* 2.96.3, 4.64.3, 4.75.3 (note *ibid.*, 2.96.3); and Thucydides *Peloponnesian War* 7.44.2. Herodotus also used the term to refer to the glory attached to particular deeds; see *The Histories* 1.30.4, 1.174.1, 3.72.2, 6.15.1, 9.75. For *lamprótēs* as renown, see Herodotus *The Histories* 2.101.1. With the exception noted above, Thucydides used this family of terms with an eye to everlasting fame; compare *Peloponnesian War* 1.138.6, 3.59.2, 6.54.2, 7.55, 7.87.5, with 2.64.5, 4.62.2, 6.16.5, 6.31.6, 7.69.2, with 7.75.6, with 6.16.3, and with 1.49.7, 2.7.1, 7.71.5, 8.67.3, 8.75.2. Also note Aristotle *Constitution of Athens* 27.3.

barbarian opponents the Persians was the fundamentally human character (*anthropophuēas*) assigned Hellas's divinities.⁵⁵ When the Greeks bowed down, they bowed down before something to which they themselves could aspire. Dionysus and Heracles, though born of mortal women, had both crossed the boundary separating men from the gods.

The poet Pindar spoke for his compatriots when he restated Homer's great theme:

There is one race of men, and one race of gods
Yet from a single mother, we both draw our breath.
A division of all power keeps them asunder.
The one is nothing—while, for the other,
Brazen heaven remains forever a safe seat.
We bear, this notwithstanding, a certain resemblance
To the immortals (*athanátois*) either in our nature
Or in greatness of mind.
Yet we do not know
To what goal
Fate has written that we,
By day and by night,
Must run our great race.⁵⁶

They too believed that a species of immortality lay within man's reach, even if it was almost always beyond human grasp. For that reason, they adopted the Heracles of Hesiod's *Theogony* and the Achilles of Homer's *Iliad* over the wily Odysseus as the models for Greek manhood.

Resplendent in victory (*kallínikos*) and renowned for defending both men and the gods against evils (*alexíkakos*), Heracles was famous for his exploits and his labors. By freeing Prometheus from his bonds and by slaying dread monsters like Geryon, the Lernaean Hydra, the Nemean Lion, and the Gigantes, "the strong son of beautiful-ankled Alcmene" transcended the merely human, achieved apotheosis, and came to "live among the immortals as a being blessed (*ólbios*): freed for all time from pain and from age." Because of these accomplishments, Heracles exercised an unparalleled sway over the Greek imagination. At least in the archaic period, he was more often celebrated in Greek poetry and art than any other figure. Throughout antiquity he was honored as a patron and chosen as a model not just by athletes but also by those whom the Greeks termed the ephebes, young men undergoing military training and a ritual initiation into the body of full citizens.⁵⁷

⁵⁵ See Plato *Republic* 10.606e; and compare Herodotus *The Histories* 2.53, with *ibid.*, 1.131. It can hardly be fortuitous that Pericles reportedly employed precisely the phrase that Plato later adopted. If Periclean Athens is to need no Homer, it is because the city itself is to be the education of Hellas. That Thucydides is in agreement with Plato regarding Homer is suggested by Thucydides' remark that the first Hellenes were those who followed Achilles; compare *Peloponnesian War* 2.41.1–4, with *ibid.*, 1.3.3.

⁵⁶ Pindar *Nemean Odes* 6.1–7.

⁵⁷ Hesiod *Theogony* 950–55. Also see *ibid.*, 289–94, 313–18, 326–32, 526–34. For a thorough survey of the evidence concerning the various ways in which poets, artists, and philosophers have treated Hesiod's hero, see G. Karl Galinsky, *The Herakles Theme* (Oxford, 1972), 1–125. Also see Walter Burkert, *Structure and History in Greek Mythology and Ritual* (Berkeley and Los Angeles, 1979), 78–98; and Susan Woodford, "Exemplum Virtutis: A Study of Heracles in Athens in the Second Half of the Fifth Century B.C." (Ph.D. dissertation, Columbia University, 1966). For the ephebes, see Athenaeus *Deipnosophistae* 11.494f; and Pierre Vidal-Naquet, "The

Homer's hero played no less important a role in the citizen's education. Taught from youth to be "a speaker of words and a doer of deeds," Achilles tried "always to be the best (*aristeúein*) and to be superior to others." He neither sought nor achieved godhead, but he was willing to embrace an untimely death in exchange for the promise of undying glory—and the men who came after would regard this defiance of necessity as the distinguishing feature of those who deserved to be free. "The best men," Heraclitus wrote, "choose one thing above all others—everlasting fame among mortals." "The many," he continued, "are easily satisfied—like cattle."⁵⁸

Heraclitus's metaphor abides.⁵⁹ But the contempt that the sixth-century Ephesian aristocrat had showered on the many the fifth-century Athenian democrat was to reserve for the barbarians. In *The Persians*, Aeschylus's chorus compares the Great King Xerxes with a herdsman and his army with "a divine flock driven over the entire earth—both by foot and on the sea." When Xerxes' mother, Atossa, asks who is the "shepherd" of the Greeks and who "serves as master (*epidespózei*) over their army," the chorus replies that the Hellenes "are said to be the slaves and subjects of no man." The coming of democracy has evidently opened up to the many the very quest for everlasting glory that Heraclitus had made the preserve of the few. Thus, when Aeschylus spoke of what has been won with the defeat of the Mede, he mentioned not only freedom from the demeaning necessity to pay tribute to a master and to bow down to a man as if to a god; he added the political dimension as well. "No longer will the tongues of mortals be held under guard," he wrote, "for the commoners (*lâos*) are released and left free (*eleúthera*) to make speech." Political liberty was nothing more and nothing less than the opportunity to do or say something of note.⁶⁰

Plato grasped this. He, too, believed that it was the yearning for renown that distinguished man from the animals. "The human race," he wrote, "possesses by nature a certain share in immortality, and the desire for this belongs naturally to all—for the desire to become famous and not to lie nameless after death is the desire for just such a thing." In *The Symposium*, Plato suggested that this longing can be satisfied in a variety of ways. Those who most resemble animals, he hints, will live solely through their offspring. But ambitious women and men like Alcestis, Achilles, and Codrus "are ready to run every risk, to spend their substance, to

Black Hunter and the Origin of the Athenian *Ephebeia*," and "Recipes for Greek Adolescence," in R. L. Gordon, ed., *Myth, Religion, and Society* (Cambridge, 1981), 147–85.

⁵⁸ Homer *Iliad* 6.208, 9.443; and Heraclitus fr. 29 (Diels-Kranz¹⁰). In the story he tells Alcinous, Odysseus represents Achilles as having repudiated after death the heroic ethic by which he was guided in life. Achilles reportedly said that he would prefer the indignity of being a thete on earth—even a thete forced to serve a man so poor that he lacked an estate—to the privilege of being king over the dead. Homer *Odyssey* 11.488–91. Note, too, Aristotle's juxtaposition of *Heracles and the Dioscuri* (heroes who achieved apotheosis) with *Achilles and Ajax* (heroes who did not); Hymn to Virtue, fr. 675 (Rose).

⁵⁹ See Aristotle *Nicomachean Ethics* 1095b19–31; and Plato *Republic* 9.581c, 9.586a–b. Also consider, in this connection, the titles and the themes of Gustave Flaubert's *Madame Bovary* and his *Bouvard et Pécuchet*. According to Tocqueville, one consequence of the emergence of commercial society may be "to reduce each nation in the end to no more than a herd of timid and industrious animals, of which the government is the shepherd"; *Democracy in America*, 2: pt. 4, chap. 6.

⁶⁰ See Aeschylus *Persians* 73–76, 241–42, 584–97. Also note Thucydides *Peloponnesian War* 1.70, 2.34–46, 2.60–64; and Plato *Laws* 3.694e–695a. In this connection, one should consider Herodotus *The Histories* 5.78.

undergo every sort of labor, and even to die for the sake of achieving a deathless fame for all time." Among the ambitious Plato included men of intelligence (*phrónēsis*) like the poets and those responsible for discoveries in the mechanical arts. But he reserved his most emphatic praise for the practitioners of the political art. As he put it, "By far the greatest and the most noble form of *phrónēsis* is that which deals with the ordering of cities and of households."⁶¹

To comprehend fully what Plato had in mind, one must recognize that the *pólis* was itself a memorial to those who had shaped it. If the classical political philosophers were in accord with their forefathers and contemporaries in supposing that politics is a pursuit endowed with great dignity, it is because they shared the popular belief that the conscious, purposive political action of the lawgiver (*nomothētēs*) can give form to a people. The Greeks understood well what Marx, Weber, and modern social scientists in general have forgotten: that the articulation of humanity into nations and political communities is of greater fundamental importance than its articulation into economic and social classes. They recognized that to make this assertion is to give primacy to politics and to deny the tyranny of circumstance. Although much may separate Plato from Aristotle, on this fundamental point they were agreed: to take politics seriously, one must be willing to suppose that the political regime (*politeía*), rather than economic or environmental conditions, determines the way of life of a people and that education in the broadest and most comprehensive sense (*paideía*) is more important than anything else in deciding the character of the regime.⁶² Thus, when Plato provided examples of the supreme practitioners of the political art, he mentioned most prominently not the lawgivers Solon and Lycurgus but the epic poets Homer and Hesiod.⁶³ The latter were the education of Hellas. They contributed more than anyone else to the ordering of its cities and households. By providing the Greeks with a common pantheon and with a shared understanding of what constitutes the good life, Homer and Hesiod formed the scattered communities of Greece into a single nation and defined the horizon within which the ancient legislators went about the

⁶¹ Plato *Laws* 4.721b–c, and *Symposium* 207a–209a. In the first passage, the emphasis is procreation, but the implications of Plato's claim are more extensive. Also see Tyrtaeus fr. 12.31–32 (West); Theognis 245–46 (West); and Cicero *Tusculan Disputations* 1.45.109–46.111.

⁶² This conviction explains the prominence of *paideía* as a theme in both *The Republic* and *The Laws*; see Plato *Republic* 2.376c–4.445a, 6.487b–497a, 7.518b–541b, 8.548a–b, 8.554a–b, 8.559b–c, 10.600a–608b, and *Laws* 1.641b–2.674c, 3.693d–701b, 4.722b–9.880e, 11.920a–12.962e. For the *politeía* as "the one way of life of a whole *pólis*," see the scholia on Plato *Laws* 1.625b. Also note Aristotle *Politics* 1295a40. In one passage, Aristotle suggested that it is the provision of a common education (*koimē paideía*)—and nothing else—that turns a multitude (*plēthnos*) into a unit and constitutes it as a *pólis*. In another, he indicated that it is the regime (*politeía*) that defines the *pólis* as such. *Ibid.*, 1263b36–37, 1276a8–b15. Though apparently contradictory, these two statements are in fact equivalent in the crucial respect: to decide who is to rule or what sorts of human beings are to share in rule is to decide what qualities are to be admired and honored in the city. That decision—more than any other—determines the *paideía*, which constitutes "the one way of life of a whole *pólis*." Compare *ibid.*, 1273a39–b1, 1278b6–15, with *ibid.*, 1295a40–b2, and see *ibid.*, 1264a24–1266b38, 1277a12–b32, 1283a3–42, 1288a6–b4, 1289a10–25, 1292b11–21, 1297a14–b34, 1323a14–1342b34. Circumstance prevails only when the citizens have allowed it to determine their *paideía*. Note the degree to which Polybius's celebrated discussion of the Roman *politeía* is, in fact, a discussion of the Roman *paideía*; Polybius *The Histories* 6.19–58. The same can be said for Xenophon's account of the Persian *politeía*; *Cyropaedia* 1.2.15. See Xenophon *Vegetal* 1.1; Plato *Republic* 8.544d–e, and *Laws* 4.711c–712a; Isocrates 2.31, 3.37, 7.14; and Cicero *Republic* 1.31.47, 5.3.5–5.7 (note *Laws* 1.4.14–6.19, 3.1.2). Also see Strauss, *Natural Right and History*, 135–38. For Marx and Weber, see notes 7, 19, 32–33, above.

⁶³ Plato *Symposium* 209a–e.

business of imposing order on the various *póleis* and their constituent parts. In the quest for the immortal, the ordinary lawgiver falls short of the epic poet. As an educator, the epic poet is destined to fall short of the philosopher alone.⁶⁴

The ordinary citizen of the *pólis* might well have queried the claim that Plato and Aristotle made on behalf of the philosopher, but the rest he took for granted. Throughout Hellas, there was general accord: an existence eked out on the periphery of things, outside the middle ground of the political arena, was a subhuman life and hardly worth living. As Aristotle put it, the city existed “for the sake of noble action”; its dignity as a moral community derived from its capacity to provide for men “the sharing of words and of deeds.” Only a handful of Greeks could seek to rival Homer and Solon, and no one supposed that every man could become an Achilles. But as a warrior band in republican form, the *pólis* offered the ordinary citizen a participation in greatness inconceivable under a monarchy, in a commercial society, or in a polity the size of a nation (*éthnos*). The existence of middle ground multiplied the opportunities to display eloquence in council and courage in battle. Because the city constituted an audience with the prospect of permanence, it provided the citizen with the hope of achieving through his contributions to its welfare at least a shadow of the eternal fame that Hesiod gave Heracles and Homer, Achilles.⁶⁵ “We should not follow those who exhort us, being human, to think human thoughts and, being mortal, to think mortal thoughts,” wrote Aristotle, “but, as far as is possible, we should make ourselves immortal (*athanatizein*) and do everything in life in accord with the most powerful thing in us.”⁶⁶ It was this straining after immortality that distinguished a human life (*bíos*), constituted by *práxis* and possibly worth recounting, from mere animal existence (*zōē*).⁶⁷ the *pólis* made the maintenance of this distinction intelligible.

The fact that the political community was itself the repository of memory served

⁶⁴ Compare Plato *Symposium* 210a–212a, with *Republic* 10.599b–608b. Theognis was the first to depict the ship of state; not surprisingly, he indicated that the poet himself rightly belongs at the helm; Theognis 667–82 (note *ibid.*, 543–47) (West). The term *ainós*—used by the poets to describe the stories, fables, and legends they recounted—denotes legislation as well. Consider Nietzsche, *Also Sprach Zarathustra*, part 1: “Vom neuen Götzen.”

⁶⁵ Aristotle *Politics* 1280b38–1281a3, and *Nicomachean Ethics* 1126b11–14. The Spartan Tyrtæus appears to have been the first to conceive of the *pólis* as an audience with the prospect of permanence; fr. 12 (West). Also see Werner Jaeger, “Tyrtæus on True Arete,” in his *Five Essays* (Montreal, 1966), 102–42. The Athenians, and Pericles in particular, soon took up his idea; see Thucydides *Peloponnesian War* 2.34–46, esp. 2.41–44, and 2.60–64, esp. 2.64.3–6; and Aristophanes *Birds* 393–99. Also see Felix Jacoby, “*Patrios Nomos*,” in his *Abhandlungen zur griechischen Geschichtschreibung* (Leiden, 1956), 260–315; Arnold W. Gomme, *An Historical Commentary on Thucydides*, 2 (New York, 1956): 94–101; and Nicole Loraux, *L’Invention d’Athènes: Histoire de l’oraison funèbre dans la cité classique* (Paris, 1981). In the fourth century, Plato’s Athenian Stranger can even hint that the city existed for this precise purpose: “In approaching the end (*télos*) for the political regime (*politeia*) as a whole,” he remarks, “the lawgiver should see in what manner it is fitting that there be funerals for each of those who have died, and he should observe what honors he ought to allocate to them.” Plato *Laws* 1.632c. Also note *ibid.*, 7.801e; and Herodotus *The Histories* 1.30. In keeping with this understanding, Cicero argued that a *civitas* should be ordered with an eye to its lasting forever; *Republic* 3.23.34. Also see Plato *Laws* 4.714a. At Rome, in a manner even more straightforward than in Greece, the family shared with the city the function of being the guarantor of commemoration; Polybius *The Histories* 6.53–55.

⁶⁶ Aristotle *Nicomachean Ethics* 1117b31–34. Although Aristotle here defended the attempt to live the philosophical life, his point applies with equal force to the quest for eternal fame. See the texts to which he was responding: Pindar *Isthmian Odes* 5.13–16; Sophocles fr. 590 (Radt); and Antiphanes fr. 289 (Edmonds). Note Aristotle’s treatment of this theme in the Hymn to Virtue, which he wrote in memory of his friend Hermias of Atarneus; fr. 675 (Rose). For the impulse behind marriage and procreation, see Plato *Laws* 4.721b–c.

⁶⁷ Life is for action (*práxis*), not production (*poiēsis*); Aristotle *Politics* 1254a7.

to distinguish freedom of speech (*lógos*) from mere freedom of expression (*phōnē*). The citizen aimed at glory, not at notoriety. He wanted to be famous forever and not merely to be a celebrity.⁶⁸ For this reason, his speech was tempered and directed by a prudent concern with the common good. Thus, when Euripides' Theseus comes to define *tò eleútheron*, he links the quest for eminence with the provision of useful advice (*tà chrēstá*). In similar circumstances, a Sophoclean character does the same. "Where one is not allowed freely (*eleuthērōs*) to give voice to *tà chrēstá*," he observes, "worse advice (*tà cheirōna*) will triumph in the city and the men will trip up its safety with their blunders." Prudence is the standard for political speech. The *pólis* of Phocylides' poem is "superior to senseless Nineveh" precisely because the opportunity for public deliberation allows its affairs to be conducted "in an orderly manner (*kosmiōs*)."⁶⁹

The subordination of the quest for immortality to the needs of the political community is perhaps most strikingly evident in the one field of endeavor that modern thought links most closely with an untrammelled freedom of expression. Euripides may have been at odds with Aeschylus over a good many things, but, if Aristophanes is to be trusted, the two were in agreement on the standard by which poetry should be judged. In *The Frogs*, when Aeschylus asks why the poet deserves to be an object of wonder, Euripides makes no mention of aesthetic imperatives. Instead, he suggests that the poets should be admired "for cleverness and for advice—and because we make the human beings in the cities better."⁷⁰ From Aristophanes' Aeschylus this reply elicits no objection—and rightly so. For there is no greater indication of the primacy accorded politics and war by the city's provision of middle ground than the example set by the poet himself: on the epitaph he wrote for his monument Aeschylus made no mention of his tragedies. These were, he elsewhere claimed, mere "slices from the banquet of Homer." When the time of reckoning came, this great tragedian simply recorded that he had been among those who had fought on Athens's behalf with the Medes in the battle on Marathon's plain.⁷¹

DESPITE THE GENERAL ACCORD that reigned throughout Greece, there were men who rejected the primacy of politics and war. In the Funeral Oration, Pericles inveighs against those with a penchant for avoiding political action (*aprágmones*), and in *The Suppliants*, Euripides' Theseus hints at the presence in Athens of men of this sort when he remarks that in a free city a man lacking the desire for eminence can choose to remain silent.⁷²

⁶⁸ The love of honor points beyond itself to the desire to be virtuous—and perhaps beyond virtue, as commonly understood, altogether; Aristotle *Nicomachean Ethics* 1095b19–31.

⁶⁹ Sophocles fr. 201b (Radt); and Phocylides fr. 4 (Diehl). For the Sophoclean passage, I have adopted the emendation suggested by Reisig.

⁷⁰ Aristophanes *Frogs* 1008–10. Also see *ibid.*, 1030–36. The two quarrel not over the duty of the poet but rather over what constitutes improving the citizens; *ibid.*, 885–1465.

⁷¹ Athenaeus *Deipnosophistae* 8.347e, 14.627c–d. Also see Vita Aeschyli 120 (Westerman); and Plutarch *Moralia* 604f.

⁷² Thucydides *Peloponnesian War* 2.40.2; and Euripides *Suppliants* 438–41. Also see Thucydides *Peloponnesian War* 2.63.2–3, 2.64.4, 6.18.6–7; and Xenophon *Symposium* 4.35. As the etymology suggests, the *aprágmon* is

The sophist Lycophron appears to have been such a man, and Aristippus, another. If Aristotle and Xenophon can be trusted, neither had much use for the middle ground provided by the public arena. Yet to the best of our knowledge no Greek thinker, not even these, ever elaborated a public-spirited political philosophy grounded in liberal principles. Lycophron did treat the *pólis* as little more than “a military alliance (*summachía*),” and he reportedly argued that the law was merely “a covenant . . . a pledge to respect each other’s rightful claims” and not an instrument “able to make the citizens good and just.” This denial that the *pólis* is a moral community certainly foreshadows the social-contract theories developed by Hobbes and his successors. But something is missing: Lycophron’s account is wholly descriptive, never prescriptive.⁷³

In the seventeenth and eighteenth centuries, those who embraced the notion of the social contract did so in order to advocate what the designers of the Great Seal of the United States called “a new order of the ages.” Among these was Britain’s Thomas Pownall, once the royal governor of the Massachusetts Bay Colony. In 1783 Pownall chose to indicate his support for the American Revolution—in a fashion that underlined the radical character of the Founding Fathers’ break with the past. He indicated his support by attacking what he termed “the grand *Desideratum* of all the ancient Legislators and Institutors of Republics.” As he put it, the great statesmen of ancient times

saw the necessity that there was of an exact conformity between the Constitution of the State and the *Species of Individuals*, the *form of the community* and [the] *nature of the basis* on which such [a] State must be founded. No such Basis was there in Nature; they therefore tried a thousand different projects to form such in Art. They forced Nature. Not finding the natural situation of men to be what it was necessary to the System of their Polity it should be, they endeavoured to make it what it never could be, but under force and violence done to nature. They destroyed or perverted all Personal Liberty, in order to force into establishment Political Freedom. While Men were taught by Pride, and by a prospect of Domination over others, to call the State Free, they found themselves cut off from, and from the use of, many of the essential inalienable rights of the Individual which form his happiness as well as freedom.⁷⁴

someone who avoids *práxis*. For a view opposed to my own, see Donald Lateiner, “‘The Man Who Does Not Meddle in Politics’: A *Topos* in Lysias,” *Classical World*, 76 (1982–83): 1–12. The suffering caused by the Peloponnesian War aggravated the tension between rich and poor Athenians, and the emergence of demagogues like Cleon and Cleophon embittered Athenian politics. As a consequence, many of the rich withdrew in disgust from the political arena in the late fifth century while others sought to overthrow the democracy. By the early fourth century (if not before), the wealthy were generally regarded as suspect by supporters of the democratic regime—and a rich man, when hailed into court on one charge or another, often found it expedient to stress that he paid his taxes and performed the liturgies imposed on him but avoided political activity altogether. I doubt very much whether the evidence Lateiner collected really justifies the view that Athenians in general came to regard politics as unsavory. Just as oligarchies denied active citizenship to the poor, so also an extreme democracy might reward the well-to-do for political quiescence. If *apragmosúnē* was thought a virtue, it was the virtue not of the rulers but of the ruled.

⁷³ Aristotle *Politics* 1280b10–12. Also see Heraclitus fr. 102; and Antiphon fr. 44 (Diels-Kranz¹⁰). In their eagerness to impute a liberal outlook to ancient democracy, some scholars have neglected to consider the missing dimension; see Karl Popper, *The Open Society and Its Enemies*, volume 1: *The Spell of Plato* (5th edn., London, 1966); and Eric A. Havelock, *The Liberal Temper of Greek Politics* (New York, 1957). For a detailed critique of Havelock’s work, see Leo Strauss, *Liberalism Ancient and Modern* (New York, 1968), 24–68. Note the degree to which orthodoxy conforms to the view advanced by Popper and Havelock; W. K. C. Guthrie, *The Sophists* (Cambridge, 1971), 135–47. R. G. Mulgan came remarkably close to isolating the source of the Popper-Havelock misunderstanding; Mulgan, “Lycophron and Greek Theories of Social Contract,” *Journal of the History of Ideas*, 40 (1979): 121–28.

⁷⁴ Pownall, *A Memorial Addressed to the Sovereigns of America* (London, 1783), 67–68.

Pownall's critique of the ancient city deserves notice because there is nothing comparable to be found in Lycophron or in any other ancient writer. In antiquity no one ever asserted "the inalienable rights of the Individual." No one ever claimed that the attempt to establish a moral community on the political plane leads to the destruction or perversion of "all Personal Liberty." The ethical perspective adopted by admirers of the American experiment and asserted by liberal political theory in general is absent from the ancient argument altogether.

What is true for Lycophron and the other ancient writers is equally true for Aristippus. There is, in fact, no evidence that Aristippus was interested in political problems at all. Yet he was a devotee of personal liberty. He reportedly argued that lying between the alternatives of slavery and rule was a middle path of freedom (*eleutheria*)—of freedom from the agony of servitude and from the burdens of political participation as well. "Not for a moment do I place myself in the ranks of those wishing to rule," Aristippus remarked,

For, in light of the great effort required to provide for oneself, it seems to me to be the height of folly not to be satisfied with this, but to take on as well the additional burden of making provision for the needs of the other citizens. How would it not be great folly for a man to give up many of the things that he wants and to incur, as head (*prostátēs*) of the city, the danger of trial if he fails to accomplish all that the city wishes? The cities think it proper to make use of their rulers just as I make use of my household slaves. For I think it proper for my servants to equip me in abundance with those things I need, but not themselves to lay hands on anything; and the cities think it necessary for their rulers to provide them with as many good things as possible and to abstain themselves from their enjoyment. Should there be those wishing to make trouble for themselves and for others, I would educate them and class them with those fitted to rule. But I would rank myself with those wishing to live as easily and as pleasantly as possible.

Socrates finds this argument unsatisfactory. It would be fine, he retorts, if one encountered no human beings on the path intermediate between slavery and rule. The strong, he warns, have a way of enslaving those who abstain from political life. For Aristippus, however, the danger of enslavement is not a serious problem. "To avoid it," he explains, "I do not lock myself up in my own city, but am a stranger (*xénos*) everywhere." When Socrates responds that the *xénos* is in all cities an easy mark with little chance of defending himself, Aristippus shifts his ground, tacitly acknowledging that the middle path of personal liberty is an illusion. He is apparently unaware that the polity itself can be refashioned to provide for the needs of those "wishing to live as easily and as pleasantly as possible." Many centuries would pass before the invention of "a new science of politics" aimed solely at the protection of life, liberty, and the pursuit of happiness.⁷⁵

The subsequent exchange between Socrates and Aristippus is even more revealing than the first. Defeated on his own ground, Aristippus attacks Socrates on his. Having been forced to concede that the path of personal freedom eventually merges with that of slavery, he argues that the distinction traditionally drawn

⁷⁵ Xenophon *Memorabilia* 2.1.1–34. Aristippus was not unique; also see Aristotle *Politics* 1324a5–1325b32, and *Nicomachean Ethics* 1179a13–16; Diogenes Laertius 2.6–7, 8.63; Plato *Hippias Major* 283a; and Plutarch *Pericles* 16.7–9. For "the new science of politics," see Tocqueville, *Démocratie en Amérique*, 1: "Introduction"; and Hamilton, *The Federalist*, no. 9. Note that for Madison, "the first object of Government" is "the protection" of the "different and unequal faculties of acquiring property"; *The Federalist*, no. 10.

between the path of slavery and that of rule cannot intelligibly be maintained. Here again, the standard he adopts is that of pleasure or, rather, the avoidance of pain. When Socrates has gone through all the evils associated with being a slave, Aristippus responds,

But Socrates! What about those educated in that royal art which you seem to equate with human flourishing (*eudaimonía*)? If they are to go hungry, to be thirsty, to endure cold, to pass sleepless nights, and voluntarily to undergo every other hardship, how does their condition differ in reality from that of men whose sufferings are forced on them? I cannot understand what difference it makes whether the victim is willing or unwilling if the same back gets flogged and the same body is besieged by all these torments—except, of course, that folly is attributable to anyone who freely chooses to endure pain.

To answer this charge, Socrates must establish that the life devoted to politics offers greater satisfaction than that devoted to pleasure. He begins by noting two advantages that the man who toils voluntarily has over the slave. Such a man can choose when and under what circumstances to cease, and, like the hunter in search of game, he works with pleasure in anticipation of reward. The slaughter of game may not be worth the effort, but other prizes are: "Consider those who labor in order that they might possess good friends, master their enemies, or gain the physical and spiritual capacity to manage their own households well, to help their friends, and to become benefactors of the fatherland. How would they not take pleasure in toil for all this—and live lives full of joy, looking on themselves with admiration and being praised and envied by others!" The man who avoids participation in public life and sidesteps the burdens of politics and war has no opportunity to exercise the faculties that give him dignity and a sense of his own worth. As a consequence, even the mundane pleasures of life lose their attraction.⁷⁶

Socrates illustrates this last point by retelling the parable in which the sophist Prodicus stages a debate between Virtue (*Areté*) and Vice (*Kakía*). When Heracles was still a young man on the verge of beginning his labors and had not yet become a god, Virtue and Vice presented the model for ephebes with a choice. *Kakía* offered him "a short and easy road to happiness (*eudaimonía*)." To her blandishments, *Areté* responded,

Wretch! What do you possess that is any good? What can you know of real pleasure—when you have no wish to do any work for pleasure's sake? You do not even wait for the desire for pleasant things to come on; you fill yourself full of everything before you even feel the need. Before feeling hunger, you eat; before feeling thirst, you drink. In order that you may take pleasure in dining, you contrive the presence of chefs; in order that you may take pleasure in drinking, you equip yourself with expensive wines and rush about in search of snow in summer; and in order that you may take pleasure in falling asleep, you provide yourself not only with soft bedding, but with a frame for your couch as well. You desire sleep—not because you have labored, but because you have nothing to do; and you force sex before it is needed, contriving everything and using men in place of women. So, you train up your friends, behaving arrogantly at night and sleeping through the most useful hours of the day. . . . You never hear praise, the most pleasant of all things to hear; and you never see the most pleasant of all things to see: for nothing is more pleasant to see than one's own noble work.

⁷⁶ Xenophon *Memorabilia* 2.1.1–34.

Socrates' point is much the same as before, but he manages here to draw attention to the emptiness and the self-contradictory nature of a life devoted to material comfort and pleasure. The short and easy road to *eudaimonía* does not lead there at all. It leads instead to an existence fraught with boredom and marked by an almost desperate search for diversion.⁷⁷

In pondering the emerging bourgeois regime of eighteenth-century Britain, Montesquieu noticed a similar phenomenon. In England, he observed, "all the passions are left free." "Each individual, always independent, follows his caprices and his fantasies." The majority care little what anyone else thinks; for that reason, they tend to "abandon themselves to their own humours." Unfortunately, this freedom, although it is as complete as anyone could ask, brings them little satisfaction. In bourgeois society the people have a "restless spirit." The men try to lose themselves in debauchery, and "the majority of those blessed with wit and intelligence (*esprit*) are tormented by that very *esprit*: in the disdain and disgust they feel for all things, they are unhappy in the midst of so many occasions for felicity." This observation is striking and strange, and it would be easy to dismiss it as yet another French discovery of *la maladie Anglaise*—were it not for the remarks made some years later by a student of commercial society no less acute.⁷⁸

Nearly a century after the publication of Montesquieu's *L'Esprit des lois*, Tocqueville paid a visit to a new nation even more thoroughly bourgeois than eighteenth-century Great Britain. On this occasion Tocqueville took note of "that peculiar melancholy which the inhabitants of democratic countries often exhibit in the bosom of abundance, and the disgust with life which sometimes seizes them in the midst of an easy and tranquil existence." In America, he wrote, "I saw the freest and most enlightened men placed in the happiest condition that there is in the world; it seemed to me as if a kind of cloud habitually covered every visage, and I thought them grave and almost sad, even in their pleasures." Tocqueville did not find the Americans prone to debauchery; indeed, he feared "much less for democratic societies from the audacity than from the mediocrity of desires." Equality tended to foster in the United States "a kind of decent materialism" aimed at wealth and comfort, but not at magnificence. The danger was not that this would corrupt but that it would "soften the soul and noiselessly unbend its springs of action."⁷⁹

Montesquieu nowhere identified the source of the peculiar unhappiness that afflicted English men of *esprit*, but for the Americans Tocqueville made the attempt. He attributed their "secret disquietude" and the danger they ran of becoming dispirited to the same two causes: to "their taste for material enjoyments" and to the "universal competition" that democratic society opens up. The first sends democratic man on a bootless chase in "the single-minded pursuit of the goods of this world," and the fact that "he has but a limited time at his disposal to find, to lay hold of, and to enjoy them. . . . fills him with care, with fears and regrets, and maintains his soul in a kind of ceaseless trepidation." The second of the two causes only

⁷⁷ *Ibid.*

⁷⁸ Montesquieu, *L'Esprit des lois*, bk. 19, chap. 27.

⁷⁹ Tocqueville, *Démocratie en Amérique*, 2: bk. 2, chaps. 11, 13.; bk. 3, chap. 19. Also see *ibid.*, bk. 3, chaps. 13, 17–18, 21.

aggravates his frustration:

When all the prerogatives of birth and of fortune are eliminated, when the professions are open to all, and a man may reach by his own efforts the summit of each, an easy and unbounded career seems to open itself up to the ambition of men, and they readily suppose that they are called to destinies grand. But this is an erroneous view which experience corrects all the days of their lives. The very equality that permits every citizen to conceive vast hopes renders all the citizens, as individuals, weak; it circumscribes their powers on every side at the same time that it permits their desires to increase. . . . When men are almost alike and all follow the same route, it is quite difficult for any one from among them to walk swiftly and cut through the undifferentiated mob (*la foule uniforme*) that surrounds and presses in on him. The constant opposition between the inclinations to which equality gives birth and the means which it furnishes for their satisfaction torments and wearies the soul.⁸⁰

For this problem, Tocqueville provided no completely satisfactory solution. He was acutely aware that, for all but a handful of men, the very size of the extended republics of modern times rules out the provision of middle ground adequate as a defined space accessible for the display of the human capacity for rational deliberation and cooperative action. He knew as well that the commercial orientation of these regimes inevitably denied that handful of politically active men the public attention they craved. In pondering ways to prevent the soul's enervation under these conditions, Tocqueville drew attention to the few opportunities that remained for the ordinary citizen to participate in the political process. In the United States, he discovered, local government was vigorous, and the citizens were free to form public associations for almost any purpose. As a consequence, where local government impinged on the interests of individuals, they were willing and able to band together to resist encroachments and to force reform. By the expedient of "self-interest rightly understood," ordinary men were drawn out of their shells and into a public arena still existent, though diminished in dignity. The citizens' concern with their own long-term interests might just infect them with a passion for public affairs less robust than the longing for glory that had animated the Greeks, but invigorating nonetheless.⁸¹

The observations of Montesquieu and Tocqueville deserve heed largely because they accord so well with the remarks of Xenophon's Socrates. Ordinary human beings born into fully bourgeois societies may not be prone to the vanity and taste for magnificence that hurl those of aristocratic temper into ever greater refinements of pleasure. But, particularly when blessed with affluence, ordinary citizens tend to wander aimlessly, lacking all purpose and afflicted with a weariness of the world. Where men are "accustomed to consider personal fortune as the sole object of care," Adam Ferguson observed,

they, who, in the vulgar phrase, have not their fortunes to make, are supposed to be at a loss for occupation, and betake themselves to solitary pastimes, or cultivate what they are pleased to call a taste for gardening, building, drawing, or music. With this aid, they endeavour to fill up the blanks of a listless life, and avoid the necessity of curing their languors by any positive service to their country, or to mankind.⁸²

⁸⁰ *Ibid.*, bk. 2, chap. 13.

⁸¹ *Ibid.*, pt. 2, chaps. 1–17.

⁸² Ferguson, *Essay on the History of Civil Society*, pt. 1, sect. 8.

The way of life advocated by Xenophon's Socrates is by no means above reproach, particularly in a world made safe for the likes of Aristippus. But for all save the women, the slaves, the metics, and the very poor, the Greek *pólis* did have one great advantage. It may not have eased the provision for mere life and for "animal enjoyment," but it did give men of *esprit* something to live for by opening up a middle ground on which they could develop and display their capacities for courage, for wisdom, and for eloquence. "Happiness," Adam Ferguson argued,

is not that state of repose, or that imaginary freedom from care, which at a distance is so frequent an object of desire, but with its approach brings a tedium, or a languor, more unsupportable than pain itself. . . . It arises more from the pursuit, than the attainment of any end whatever; and in every new situation to which we arrive, even in the course of a prosperous life, it depends more on the degree to which our minds are properly employed, than it does on the circumstances in which we are destined to act, on the materials which are placed in our hands, or the tools with which we are furnished.⁸³

If in echoing Aristotle Ferguson is right, his remarks go a long way toward explaining the strangest omission from the ancient Greek language: the lack of a specific term for what Finley Peter Dunne's Mr. Dooley would have called boredom.

⁸³ *Ibid.*

La Salle: Discovery of a Lost Explorer

PETER H. WOOD

Such is the lot of those men whom a mixture of great faults and great virtues draws from the common sphere . . . ; their acts prove beneficial to some, and harmful to others; the latter take revenge by decrying them without moderation; the former exaggerate their merit. Hence the different portraits drawn of them, none of which is an exact resemblance; and as hatred and the habit of evil speaking always go further than gratitude and friendship, and as calumny finds an easier credence with the public than commendation and praise, the portrait of the Sieur de la Salle was more disfigured by his enemies than embellished by his friends.

—Charlevoix, *Histoire de la Nouvelle France*¹

IN THE SUMMER OF 1684, René-Robert Cavelier, Sieur de la Salle, set sail from France bound for the lower Mississippi, which he had explored from the north two years before, but his elaborate seaborne mission ended in failure and death. Just as a century earlier John White's "Lost Colony" presaged the successful English occupation of Virginia, La Salle's expedition to the Gulf of Mexico pointed the way for subsequent French settlement in Louisiana. Yet, while the colony sent by Sir Walter Raleigh to the Cape Hatteras region has been frequently studied and lavishly commemorated, even the location of La Salle's outpost in East Texas remains obscure.² Moreover, an elaborate but flawed representation of the explorer and his colonizing plan has gradually taken hold over the years. The three-hundredth anniversary of his final voyage seems an appropriate time to offer a new interpretation of his thoughts and actions.

¹ Pierre de Charlevoix, *Histoire et description generale de la Nouvelle France avec le journal historique d'un voyage fait par ordre du roi dans l'Amérique Septentrionale*, 2 (Paris, 1744): 286–87.

² See Herbert E. Bolton, "The Location of La Salle's Colony on the Gulf of Mexico," *Mississippi Valley Historical Review*, 2 (1915): 165–82; Kathleen Gilmore, *The Keeran Site: The Probable Site of La Salle's Fort St. Louis in Texas*, Texas Historical Commission, Office of the State Archeologist Reports, no. 24 (Austin, 1973); and, most recently, Robert S. Weddle, "Spanish Search for French Fort: A Path to Danger and Discovery," *American West*, 20 (1983): 26–34. Although Garcitas Creek near Matagorda Bay is now generally accepted as the site of the colony, see the argument for a more southwestern location in H. Dickson Hoese, "On the Correct Landfall of La Salle in Texas, 1684," *Louisiana History*, 19 (1978): 5–32.

Twentieth-century historiographic trends suggest in part why La Salle and his endeavor have been misunderstood. Skills associated with old-style geographical history and biography have fallen into relative disuse. Most historians of colonial North America now renounce or ignore the naive and Eurocentric tales of empire spun by their predecessors. And few, as yet, have turned their critical attention to the rich social, environmental, and intercultural questions embedded in most narratives of exploration. Moreover, a persistent disregard for non-English persons and non-Eastern regions has long characterized the field and, despite occasional gestures to the contrary, still dominates the narrowed mainstream of early American history.³ But a further cause for cumulative distortion of the historical La Salle lies in the documentation of his expeditions. From the start, intrigue and uncertainty affected the record of the explorer's journeys. Protagonists vied for honor; copyists transcribed errors; partisans dismissed evidence; lobbyists revised reports; printers plagiarized narratives. A handful of the Texas colonists, unlike the Roanoke settlers, survived to relate their tales to Spanish captors or French officials, but these partial and sometimes misleading accounts remain hard to reconcile.⁴ Subsequent generations of interpreters, far from resolving these discrepancies, managed to compound the confusion. Maps and letters were torn or misplaced; transcribers and translators introduced new readings for old passages; editors italicized some sentences and omitted others to stress their own interpretations of key texts. Into this tangled wilderness plunged intrepid biographers, rather as La Salle himself had once set out, "loaded with baggage . . . sometimes pushing through thickets, . . . sometimes wading whole days through marshes . . . resolving to go . . . to learn for myself."⁵ And in much the way that La Salle in his travels fell victim to plausible but mistaken interpretations that multiplied into fatal errors over time, so too those attempting to recount his mission have been led astray by a succession of small misinterpretations.

Indeed, over the past century the saga has taken on the aura of a morality play. Gradually a stock portrayal of La Salle has developed that depicts the explorer as either a self-deluded madman or a calculating Machiavelli, and the more troubled he is made to seem, the more engrossing the fictional drama becomes. Each revival of the tragedy allows a new scholar-director to re-examine the commonly accepted theme of "La Salle's madness." Was he a pitiable braggart like Falstaff, "babbling of green fields"? Or was he a tormented patriarch like Lear, knowing "that way

³ Ert J. Gum, "La Salle and the Historians," in Archie P. McDonald, ed., *East Texas History: Selections from the East Texas Historical Journal* (Austin, 1978), 39. For a useful introduction to the historiography of the Gulf Coast, see Samuel Proctor, "Bibliographic Resources in the United States for Gulf Coast Studies," in Ernest F. Dibble and Earle W. Newton, eds., *In Search of Gulf Coast Colonial History* (Pensacola, 1970), 46–76.

⁴ One of the fullest surviving journals is Henri Joutel's *A Journal of the Last Voyage Perform'd by Monsr. de la Sale, To the Gulph of Mexico, To find out the Mouth of the Missisipi River* (Paris, 1713; English trans., London, 1714, reprint edn., 1968). Joutel told the cartographer Guillaume Delisle in 1703 that several persons had published accounts of the expedition, but, he remarked, "they tell untruths in many passages, and the public, not having been on the spot, is unable to judge what is true from what is false"; Jean Delanglez, ed., *The Journal of Jean Cavalier: The Account of a Survivor of La Salle's Texas Expedition, 1684–1688* (Chicago, 1938), 15. Compare Robert S. Weddle, *Wilderness Manhunt: The Spanish Search for La Salle* (Austin, 1973), 254–55.

⁵ Pierre Margry, ed., *Découvertes et Établissements des Français dans l'Ouest et dans le Sud de l'Amérique Septentrionale*, 6 vols. (Paris, 1876–86), 2: 50, translated in Francis Parkman, *La Salle and the Discovery of the Great West* (1879; Frontenac edn., Boston, 1907), 189–90.

madness lies"? Or was he, like Hamlet, crazy as a fox, driven to dangerous dissembling by his own quick mind and the apparent intrigues of the court around him?⁶ Each version has its own particular logic and partial documentation, and the sweep of the action serves to overshadow most inconsistencies. No two renditions are exactly the same, yet the important scenes in this five-act epic remain surprisingly fixed, even in the most varied productions. A few quotation-filled paragraphs provide a composite version of the story line as it has been passed on to us.

Act I, involving La Salle's education in France and his early separation from the Jesuit order, and Act II, recounting his ambition as a young man in Canada to find new trade routes west and south of the Great Lakes, need not concern us here. Act III focuses on the exploration of the lower Mississippi, the so-called Colbert, in 1682, and it ends with a climax, as the central act of a tragedy should. Portrayals of this discovery scene usually follow Francis Parkman's famous nineteenth-century description. According to his vivid rendition, on April 9, La Salle raised a cross near the mouth of the Mississippi and laid claim to a vast region of America for the king of France, "and all by virtue of a feeble human voice, inaudible at half a mile." This deed marks a turning point in the drama, for standard treatments of La Salle hint that at the base of the Mississippi the "far-seeing, ambitious and perhaps paranoiac explorer" began to overreach himself, losing his mental and moral bearings almost as if he were Werner Herzog's Aguirre descending the Amazon.⁷

Act IV begins with La Salle's return upriver and a further sojourn in the wilderness, and it concentrates upon his eventual return to Europe at the end of 1683, centering on themes of deceit and secrecy amid the French court. Most narrators suggest that the explorer's insecurity and mental agitation increased in Paris. "Even while La Salle feared and berated imaginary enemies, he cast his lot with two theory-spinning abbés who meant him no good"—Eusèbe Renaudot and Claude Bernou. For several years these two had been promoting the "comic opera-like invasion scheme" of "the Peruvian turncoat" Diego de Peñalosa who wanted to attack the Spanish silver province of New Biscay west of the Gulf of Mexico. La Salle collaborated with them "to concoct" a "crazy" and "bizarre" plan not only for colonizing the Mississippi but also for mobilizing some fifteen thousand Indians from the interior to assist in conquering the rich mines of northern Mexico. Early in 1684 they described this "madcap enterprise" in several private memoirs that were no more than a "glittering lure to attract the eyes" of Louis XIV and his minister,

⁶"It is not easy to conceive how intelligent writers have exalted a man of such utter incapacity into a hero"; John Gilmary Shea, *The Expedition of Don Diego Dionisio de Peñalosa, . . . With an account of Peñalosa's projects to aid the French to conquer the Mining Country in Northern Mexico; and his connection with Cavalier de la Salle* (New York, 1882), 22. Also see William R. Taylor, "That Way Madness Lies: Nature and Human Nature in Parkman's *La Salle*," in Reuben A. Brower and Richard Poirer, eds., *In Defense of Reading* (New York, 1962), 256–81; and Louis de Vorsey, Jr., "The Impact of the La Salle Expedition of 1682 on European Cartography," in Patricia K. Galloway, ed., *La Salle and His Legacy* (Jackson, Miss., 1982), 76.

⁷Parkman, *La Salle*, 308; and W. P. Cumming *et al.*, *The Exploration of North America, 1630–1776* (New York, 1974), 36. Herzog's modern German film classic, *Aguirre: The Wrath of God* (1973), is based loosely on the sixteenth-century Amazon expedition organized by Gonzalo Pizarro and bears a fascinating relation to biographies of conquest such as Parkman's *La Salle*. For contrasting visual representations of La Salle claiming Louisiana, see George Catlin's 1847–48 sketch in Roger B. Stein, *Seascape and the American Imagination* (New York, 1975), 53; and the commemorative postcard issued in 1982 by the U.S. Postal Service.

Seignelay. According to standard accounts, the memorials contained “the deliberate distortion of the course of the Lower Mississippi by La Salle and his supporters who, in order to promote the Louisiana settlement scheme . . . , claimed that the Mississippi entered the Gulf of Mexico some 400 or 500 miles west of its true outlet.” This was a “madly devious game to be playing with an absolute monarch of Louis XIV’s stamp,” for it was bound to become “obvious . . . that La Salle had acted opportunistically and somewhat dishonestly.” But, as the story goes, this deception worked, and in April 1684 the king granted La Salle the permission and ships needed to make good his claim of empire, though the exact orders were shrouded in secrecy. As departure drew near the explorer displayed an indecisive and “increasingly frenetic attitude towards all with whom he had to deal.”⁸ Finally, amid bickering between La Salle and Captain Beaujeu, four ships set out in mid-summer with several hundred soldiers and colonists aboard.

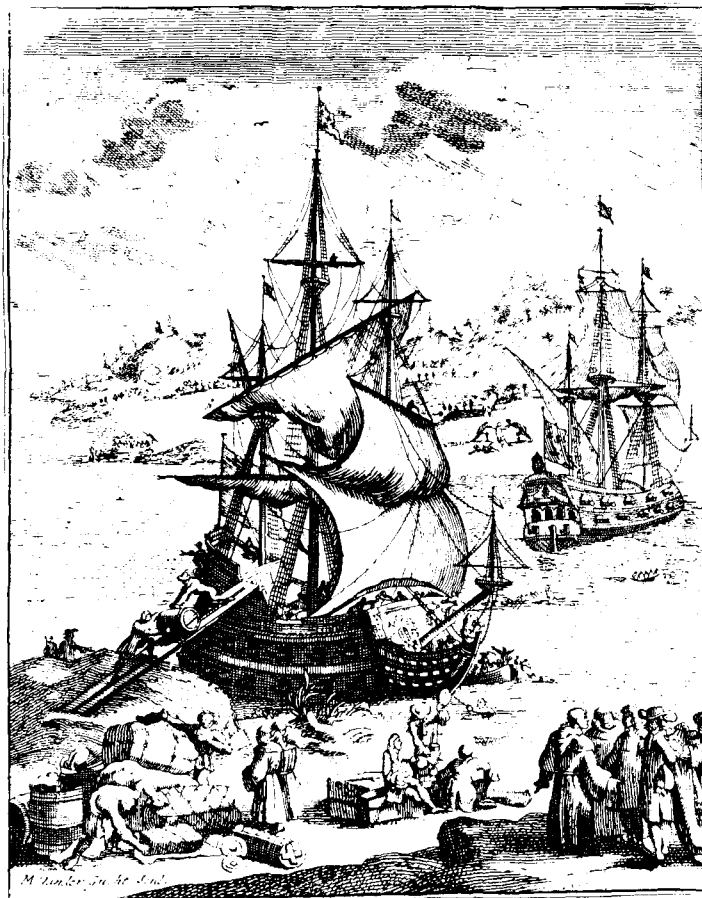
The French encountered a series of disheartening setbacks—delays, illnesses, and the loss of one boat to corsairs off Santo Domingo.⁹ In December 1684, La Salle directed his three remaining vessels into forbidden Spanish waters, and the final scenes of the tragedy were played out on the mysterious shores of the Gulf of Mexico. Act V, though covering several years, is usually presented as a swift demise, hinging on what Parkman called “A Fatal Error” and “A Vain Search.” Mysteriously, “For reasons which have never been fully explained, the mouth of the Mississippi was missed.”¹⁰ Most writers speculate as to whether this disaster occurred through tragic misfortune or human design, and many provide a map estimating La Salle’s route through the gulf to dramatize how breathtakingly close to the actual delta his course supposedly lay.¹¹ The boats of La Salle and Beaujeu became separated while searching the coast, and only after several weeks were the

⁸ Jean Delanglez, *Some La Salle Journeys* (Chicago, 1938), 99; de Vorse, “Impact of the La Salle Expedition,” 74, 71; E. B. Osler, *La Salle* (Don Mills, Ontario, 1967), 165, 168; Parkman, *La Salle*, 350, 349; Cumming *et al.*, *Exploration of North America*, 40; Osler, *La Salle*, 165; and William R. Taylor, “A Journey into the Human Mind: Motivation in Francis Parkman’s *La Salle*,” *William and Mary Quarterly*, 3d ser. [hereafter, WMQ], 19 (1962): 232. For brief profiles of Renaudot and Bernou, see Osler, *La Salle*, 158–59. For different versions of Peñalosa’s background, see the eight-part series by F. V. Scholes, “Troublous Times in New Mexico, 1659–1670,” *New Mexico Historical Review*, 12 (1937): 134–74, 13 (1938): 63–84, 15 (1940): 249–68, 369–417, and 16 (1941): 15–40, 185–205, 313–27; C. W. Hackett, “New Light on Diego de Peñalosa: Proof that He Never Made an Expedition from Santa Fe to Quivira and the Mississippi River in 1662,” *Mississippi Valley Historical Review*, 6 (1919–20): 313–35; Cesáreo Fernández Duro, *Don Diego de Peñalosa y su descubrimiento del Reino de Quivira* (Madrid, 1882); and Shea, *Expedition of Don Diego Dionisio de Peñalosa*. When the memorials for the proposed expedition were written, Seignelay had just succeeded his father, the famous Colbert, who died in September 1683. Colbert was a supporter of La Salle’s earlier exploration. Parkman wrote of La Salle’s plan, “Unless we assume that his scheme of invading Mexico was thrown out as a bait to the king, it is hard to reconcile it with the supposition of mental soundness.” He added, “It is difficult not to see in all this the chimera of an overwrought brain, no longer able to distinguish between the possible and the impossible.” Parkman, *La Salle*, 362–63.

⁹ “La Salle now lost confidence in his own plans. . . . It had been perhaps relatively easy to present the court with memoirs and maps showing how close the Mississippi ran to New Spain, but in Santo Domingo reality began to stare him in the face”; Henry Folmer, *Franco-Spanish Rivalry in North America, 1524–1763* (Glendale, Calif., 1953), 158.

¹⁰ Parkman, *La Salle*, 366; and Bolton, “The Location of La Salle’s Colony,” 166. La Salle’s supposed “ulterior reasons for making the notable mistake and thus landing in Texas rather than in Louisiana” are expounded by Delanglez in *Journal of Jean Cavalier*, 3, and *Some La Salle Journeys*, 92–99.

¹¹ See, for example, Osler, *La Salle*, 182; Paul Chesnel, *History of Cavalier de la Salle* (New York, 1932), endpapers; Marc de Villiers, *L’Expedition de Cavalier de La Salle dans le Golfe du Mexique, 1684–1687* (Paris, 1931), 78; and Marion A. Habig, *The Franciscan Père Marquette: A Critical Biography of Father Zénobe Membre, O.F.M., La Salle’s Chaplain and Missionary Companion*, Franciscan Studies, no. 13 (New York, 1934), 144.



The unfortunate adventures of Monsr de la Salle.

Figure 1: Fanciful portrayal of La Salle's colonists disembarking near Matagorda Bay in 1685. Line drawing reproduced from Louis Hennepin, *A New Discovery of A Vast Country in America, . . . Extending above Four Thousand Miles, Between New France & New Mexico, . . . Giving an Account of the Attempts of the Sieur de la Salle upon the Mines of St. Barbe, &c. . . .* (London, 1699).

ships reunited slightly south of Matagorda Bay. At that point La Salle chose to disembark, though whether he intended first to seek the river or to campaign against the Spanish has divided his interpreters.¹² After the beleaguered colonists witnessed the wreck of their supply ship on a shoal and watched Captain Beaujeu, his assignment completed, turn back to France, they moved inland to erect Fort St. Louis, looking anxiously for signs of the Mississippi.

¹² In some accounts of the coastal search, a reckless La Salle gives cautious Beaujeu the slip; in others, a confused La Salle is deserted by devious Beaujeu; in still others, the natural forces of wind and fog intervene. For a partisan summary, see Delanglez, *Journal of Jean Cavelier*, 136–37 n. 18. Delanglez noted that several modern authors have called the rumor of an operation against New Biscay “preposterous,” and he commented, “An assault upon the Spaniards under the circumstances with a handful of soldiers may be readily admitted as preposterous, but was not the entire scheme upon which La Salle had embarked, absurd? One has not to read far in La Salle’s letters in order to find other preposterous statements.” *Some La Salle Journeys*, 94–95 n. 135. For contrasting evidence, see Charles Wilson Hackett, trans. and ed., *Historical Documents Relating to New Mexico, Nueva Viscaya, and Approaches Thereto, to 1773*, 2 (Washington, 1926): 49.

"The rest of the story may," as one typical account puts it, "be told in a few words." Once La Salle "became thoroughly conscious of his isolated and highly precarious position in an unknown part of America, the imperativeness of finding the Mississippi struck home to him. Leaving settlers and artisans at his new base, he sallied forth with his soldiers to find 'his river.' He made three futile journeys inland for this purpose," though just where he searched and why remains vague. Finally, he set out on a desperate trip "to seek help from Canada," but a mutiny and ambush by several of his own men put an end to "the wanderings of La Salle." As for his followers, "the generally accepted version is that through disease and conspiracy the colonists melted away, and that finally the tomahawk silenced the last soul in the miserable Fort of St. Louis."¹³

WHATEVER THE AUTHENTICITY OF THIS COMPOSITE SKETCH, its dramatic possibilities are obvious. Nevertheless, a simpler, less flamboyant reading of the evidence seems long overdue. To begin this process, we must go back to 1844 when Harvard historian Jared Sparks launched the second series of the "Library of American Biography" with his *Life of Robert Cavelier de La Salle*. This first modern portrait apparently made an impression on young Francis Parkman, then a senior at the college. He later wrote to Sparks, "The life and character of La Salle are to me objects of especial interest," and the elder scholar, burdened with the presidency of Harvard, turned over to Parkman unused documents he had collected relating to the explorer. This material appeared in Parkman's extremely popular book *The Discovery of the Great West*, published in 1869, and in the expanded version printed ten years later under the title *La Salle and the Discovery of the Great West*.¹⁴

In both of his publications, Parkman gave the same renowned description of the moment in 1682 when La Salle first reached the gulf from Canada and America's heartland "passed beneath the sceptre of the Sultan of Versailles." But, in comparison with his former professor's less poetic account of the same incident, one small detail had disappeared. According to Sparks, "By some rude instrument La Salle had observed the elevation of the pole at the mouth of the Mississippi, and had made the latitude full two degrees too far south," putting it at 27 degrees north. Yet Parkman omitted any reference to the claimed latitude, which was off by two degrees (or nearly 140 miles). As a stylist, he may have given precedence to literary considerations. Or, since he was emphasizing La Salle's "inclination for the exact sciences" and "the modern world of practical study," he may have chosen to downplay a measurement so incorrect that it appeared at best mistaken and at

¹³ William Edward Dunn, *Spanish and French Rivalry in the Gulf Region of the United States, 1678-1702* (Austin, 1917), 34; Delanglez, *Journal of Jean Cavelier*, 3; Osler, *La Salle*, 225; and Walter Flavius McCaleb, "Some Obscure Points in the Mission Period of Texas History," *Texas State Historical Association Quarterly*, 1 (1898): 217. Some historians of La Salle have found "in his ultimate undoing tragic dignity," while others have underscored the "madness, defeat, and humiliation" of his final martyrdom; Taylor, "Journey into the Human Mind," 237.

¹⁴ Wilbur R. Jacobs, ed., *Letters of Francis Parkman*, 2 vols. (Norman, 1960), 1: 91-92; and Parkman, *The Discovery of the Great West* (1869; 7th edn., Boston, 1874), viii-ix. Parkman's epic owes more to Sparks's narrative than has been recognized. For the correspondence of Parkman with the French archivist Margry, see John Spencer Bassett, ed., "Letters of Francis Parkman to Pierre Margry," *Smith College Studies in History*, 8 (1923): 123-208; Jacobs, *Letters of Francis Parkman*, 1: 125-26, and 2: *passim*; and Louis Philippe Cormier, ed., *Lettres de Margry à Parkman, 1872-1892* (Ottawa, 1977).

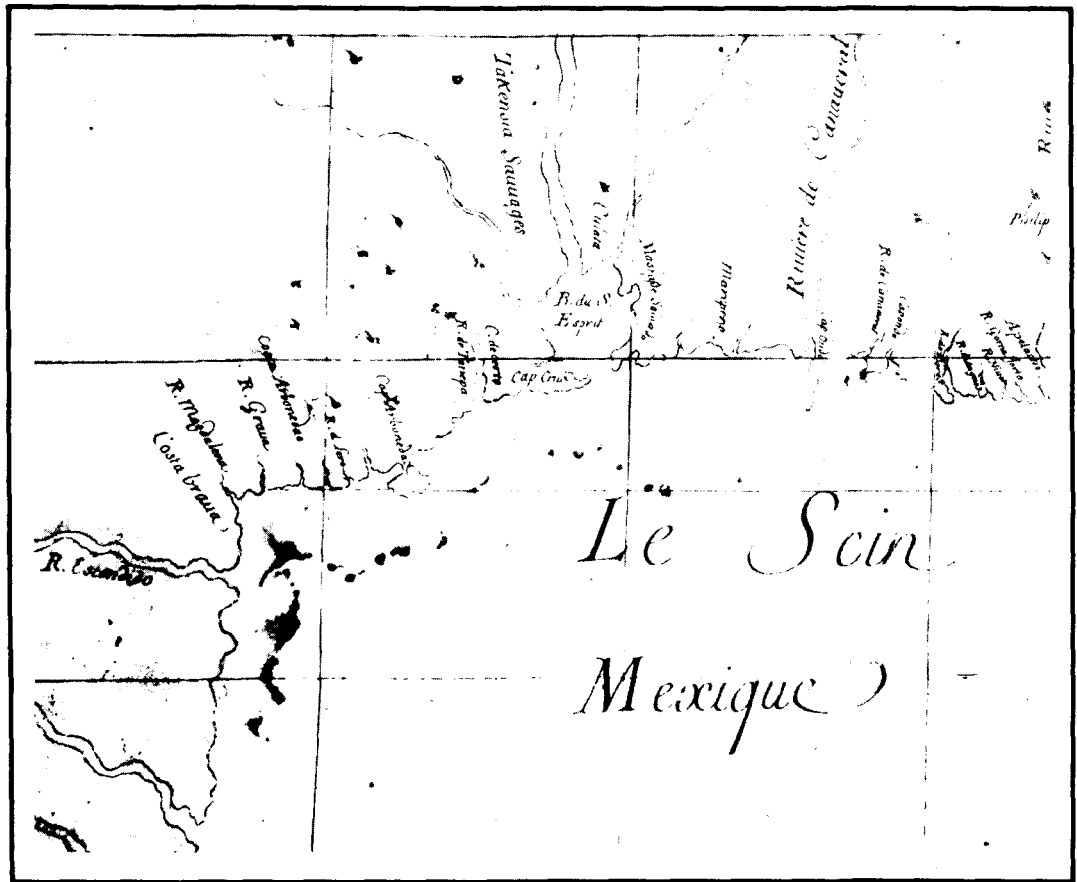
ascertain his position by analyzing the maps of the time and by comparing their general features to his own observations, especially his measurements of latitude made with an astrolabe. We can only guess which maps La Salle studied over the years or carried with him on his travels, but we know that most of the ones available (see Map 1) shared half a dozen features that he must have examined and discussed repeatedly.

1. The north coast of the Gulf of Mexico was placed (accurately) at about 30 degrees north latitude.
2. This coast ran roughly east-west, showing no sign of the enormous Mississippi Delta, which protrudes south a full degree into the gulf to 29 degrees.
3. Toward the coast's western end, the huge, fictitious Bay of Espíritu Santo, rivaling the Chesapeake in size (and seeming to combine traits of what later became known as Galveston and Mobile Bays), pushed north to roughly 31 degrees and had several large rivers emptying into it.
4. East of this bay, several other rivers flowed south into the gulf.
5. On the opposite side of the bay, where the coastline curved southwest toward Mexico (and below a vague chain of mountains from the north), a series of rivers flowed southeast and east into the gulf.
6. One of these rivers, the Escondido, was often particularly prominent, with a large entrance to the gulf near 27 degrees (and with forks upstream to the north and west that gave it features similar to those of the actual Rio Grande).¹⁷

La Salle's acquaintance Hugues Randin (often cited as Hughes Raudin), engineer for Governor Frontenac during the 1670s, had prepared a chart containing most of these features. In the lower left corner (see Map 2), Randin speculated that the

of the *Illinois State Historical Society*, 1910, 15 (1912): 129–36. But mistakes in La Salle's calculations of longitude did not determine the outcome of his southern designs, for, like all explorers before the improvement of the chronometer in the eighteenth century, he did not *expect* to be able to gauge accurately the east-west location of an uncharted site. La Salle and his European contemporaries were well aware of the huge constraints this situation created (and of the importance it imposed on measurement of latitude). The French Académie Royale des Sciences, founded in 1666, was already giving high priority to the pressing problem of accurate longitude calculation, and the British Board of Longitude was established in 1714 specifically to promote similar study; see Derek Howse, *Greenwich Time and the Discovery of Longitude* (New York, 1980), chaps. 1–2. La Salle did estimate the distances on the separate legs of his 1681–82 journey inland, and he concluded that the mouth of the Illinois where he entered the Mississippi was 24 degrees west of Quebec. But this was a crude guess influenced by distorted contemporary maps; the actual distance is 19 degrees. This estimate was given in Margry and noted by Steward. See Margry, *Découvertes et Établissements des Français*, 2: 248; and Steward, "La Salle a Victim to His Error in Longitude," 132. Steward misread the calculation as applying to the position of the mouth of the Mississippi rather than the mouth of the Illinois.

¹⁷ For historical representations of the gulf region, see Adrian Johnson, *America Explored: A Cartographical History of the Exploration of North America* (New York, 1974). For additional maps and a discussion of explorers' methods for determining their geographic position in the seventeenth century, see Cumming *et al.*, *Exploration of North America*, 13–24. Also see Jean Delanglez, *El Rio del Espíritu Santo: An Essay on the Cartography of the Gulf Coast and the Adjacent Territory during the Sixteenth and Seventeenth Century*, Catholic Historical Society, Monograph Series, no. 21 (New York, 1945), esp. chap. 7. In 1675, the Bishop of Cuba, after touring the Catholic missions of Florida, reported that the Choctaw and Mobile Indians lived "near the harbor of Spiritu Santo"; *Smithsonian Miscellaneous Collections*, 95 (1936): 10.



Map 2: Close-up of northwestern Gulf Coast from Hugues Randin's map of North America, drawn during the 1670s. In Canada, French hopes of finding a river that flowed west to the Pacific were giving way to speculation that the newly "discovered" river of Marquette and Jolliet might provide access to the Spanish-controlled Gulf of Mexico. Randin's chart reflected this possibility and was almost certainly known to La Salle (see note 18, below). The portion reproduced here shows a prominent "R. Escondido" and smaller "R. Magdalena" in the west, while the broad Mississippi is projected as flowing south—with few curves—into the fabled Bay of St. Esprit. Map from the Yale Map Collection, Sterling Memorial Library, reproduced courtesy of Yale University, New Haven, Connecticut.

Mississippi, which Marquette and Jolliet explored to the mouth of the Arkansas in 1673, must flow to the gulf via the Bay of Espíritu Santo (or "Baye du St. Esprit," as the French called it), several hundred miles northeast of the next major river, the Escondido.¹⁸ Even if La Salle and his lieutenants did not take a copy of Randin's chart with them, it represented the geography they carried in their heads as they proceeded down the Mississippi. The explorers must have anticipated that they

¹⁸ La Salle had known Randin at least since 1673 when they worked together laying out Fort Frontenac; Parkman, *La Salle*, 92, 481. Randin's little-known "Carte de l'Amerique septentrionale depuis l'embouchure de la Riviere St. Laurens jusques au Sein Mexique," now in the John Carter Brown Library, was prepared for Governor Frontenac to consolidate French knowledge of the interior. The complete map and informed comments on Randin's name and career appear in Sara Jones Tucker, comp., *Indian Villages of the Illinois Country*, Illinois State Museum Scientific Papers, vol. 2, pt. 1 (Springfield, Ill., 1942), 3, plate vi. La Salle also knew Father Louis Hennepin, whose "Carte de la Nouvelle France et de la Louisiane Nouvellement decouverte," published in Paris in 1683, was largely compiled from standard charts made before La Salle's river journey and shows the mouth of the Bay of St. Esprit and the northern Gulf Coast *above 30 degrees*, at 31 degrees north latitude. Randin's chart, in contrast, puts the same features *well below 30 degrees*, at about 28 degrees 20 minutes. La Salle, as we shall see, must have worried repeatedly about such inconsistencies from 1682 until his death in 1687.

would reach the vicinity of the large bay at roughly 31 degrees; then, passing through it or near it, they would surely encounter the Gulf Coast at approximately 30 degrees. Henri de Tonti, who shared fully in the journey, wrote, "M. de la Salle had always believed that this river discharged itself into the Baye du Saint-Esprit, but, when he had taken our latitude with his astrolabe," at the Taensa village (in the vicinity of modern-day Lake St. Joseph, Louisiana), "and found it to be thirty-one degrees" without having encountered any bay. "this led him to believe that we were in the Abscondido [Escondido] River, which was later found to be correct."¹⁹

How did La Salle and his most trusted associate come to suspect that their Colbert, or Mississippi, might be the river shown on maps as the Escondido? The answer lies not in wishful thinking or collusion but rather in their sensible observations of the river's lower course during the 1682 descent. They correctly recorded the Arkansas River intersecting the elliptical Yazoo Basin from the west. Further south they noted the precise location of major Natchez villages.²⁰ But four basic observations shaped La Salle's interpretation of the journey and therefore had a crucial influence upon his later expedition to the gulf from France; they involved the length and direction of the river and the general nature and specific latitude of its mouth. Given the conditions, all of these observations were difficult. Furthermore, each judgment—or misjudgment—is inextricably entwined with the other three in any effort to explain the exploration down the river. Since any one of these calculations could have exerted a deciding influence over how the remaining determinations were figured and assessed, each of these geographical appraisals by La Salle deserves a thorough re-examination.

Gauging the overall length and direction of the Mississippi in 1682 was less simple than it might now seem to armchair explorers, for below the Ohio, and more so below the Arkansas, the river becomes awesomely convoluted. As its volume grows and its rate of descent to sea level declines, the frequency of the serpentine turns in the riverbed increases. One huge horseshoe bend flows into another, and the boat that follows a single oxbow curve from end to end may travel three to six times further than the crow that flies directly from the top of the bow to the bottom. In other words, a long day's journey by water amounts to a far shorter advance in terms of overland distance.²¹ La Salle understood this condition all too well, having grown up at Rouen on one of the huge loops of the lower Seine as it meanders from Paris to the sea. Yet no matter how much he discounted in distance for the persistent winding of the river, he still underestimated the difference between the full length of his circuitous journey by water and the far shorter overall land distance that the Mississippi traverses before reaching the sea. He may have mistaken the speed of the current and the length of the curves and therefore believed that he had traveled farther than he had. Or he may have failed to

¹⁹ Delanglez, *El Rio del Espíritu Santo*, 103–04; and Margry, *Découvertes et Établissements des Français*, 1: 602. It is significant for the disputes regarding La Salle's astrolabe readings that the actual location of this site, between Vicksburg and Natchez in Tensas County, is close to 32 degrees. See Jeffrey P. Brain, "La Salle at the Natchez: An Archaeological and Historical Perspective," in Galloway, *La Salle and His Legacy*, 53–54.

²⁰ Patricia K. Galloway, "Sources for the La Salle Expedition of 1682," in Galloway, *La Salle and His Legacy*, 29; and Brain, "La Salle at the Natchez," 57–58.

²¹ On March 18, 1699, his Indian guides showed Iberville a portage that would shorten his journey "by one day's travel"; Margry, *Découvertes et Établissements des Français*, 4: 174; and Richebourg Gaillard McWilliams, trans. and ed., *Iberville's Gulf Journals* (University, Ala., 1981), 66.

perceive how tightly wound the coils of the river become and thus thought it covers a far greater straightline distance than it does. Guesses at the length of the river by expedition members varied, and we cannot be certain whether their point-to-point estimates referred to the curling channel of the river or to a straighter overland line. But clearly La Salle judged the distance from the Illinois to the mouth of the Mississippi to be considerably greater than it is. So he had to squeeze this lengthy river and greatly exaggerate the overall bend of its lower course to make it fit the accepted outlines of the continent.

As to the direction of the river, La Salle rightly sensed that the flow from the point where he entered it, despite constant gyrations, was generally south and southwest.²² But he gradually realized that this course did not continue as Randin's map had suggested. Instead, after winding past the entrance of the Red River (which he named for Seignelay), the mighty stream changed its orientation. As La Salle proceeded through the Atchafalaya Basin below the Taensa villages, he observed correctly that the river "at this latitude flows steadily eastward, or at most southeastward." And, as he continued in this new direction, evidence mounted that the gulf did not lie to the south as he expected. La Salle recalled later, "After we reached 31°, all the Indians who go to the sea to make salt agreed in saying that the sea was situated to the east." In confirmation, he also noted that as they neared their destination, "Every morning we saw sea mists rising in the east and moving in toward the west, even against the wind."²³ Realizing that he had traveled further than he had anticipated and was approaching the sea in a southeasterly direction, La Salle already may have begun to wonder if he was west of the Gulf of Mexico.

The explorer's observations of the delta region did little to clarify his position. In 1682 La Salle could not have surmised the unusual size and shape or even the existence of the Mississippi Delta, although he realized that he was in a low-lying country where sediment was deposited steadily. Moving diagonally across the great delta in early spring, he did recognize—more clearly than many who followed him—the annual threat posed by high water. "M. de la Salle always told us," Joutel recalled later, "that the Mississippi must be ascended nearly sixty leagues to find a place for settlements, since the lower part . . . was uninhabitable owing to floods and mud." And when he reached the Birdfoot Subdelta on April 6, where the

²² Margry, *Découvertes et Établissements des Français*, 2: 248. Although Delanglez and others accepted the idea that La Salle lost his compass early in the journey, I have not found any source beyond de Villiers's undocumented comment that La Salle "eut perdu sa boussole en descendant la riviere des Illinois." Did de Villiers mean the phrase literally, or, given his skeptical view of La Salle, did he use *perde la boussole* in its idiomatic sense, "to become disoriented or unhinged, to go off the deep end"? De Villiers, *L'Expédition de Cavelier de La Salle*, 30. The difficulty that La Salle's party had determining overall direction on the constantly bending Mississippi is illustrated by the recollections of two young men who traveled with the explorer. Several years after the expedition, they told the French engineer Minet that while descending the river with La Salle they had almost always seen the sun set in front of them; Minet, "Voyage fait du Canada par dedans les terres allant vers le sud, dans l'anne 1682," MS journal for 1684–85, Public Archives of Canada, Quebec. Minet compiled his account of the 1682 descent of the Mississippi during the 1684 voyage from France to the gulf, and he based it on discussions with several men who had made the journey. For more on this document, see Victorin Chabot, "Journal inédit relatant les expéditions de Cavelier de La Salle," *Archivist*, 8 (1981): 8–9; and Galloway, "Sources for the La Salle Expedition of 1682," 16–17. A translation of Minet's journal, with an introduction by Galloway, will appear in a forthcoming book of documents concerning the La Salle colony, to be edited by Robert S. Weddle and Mary Christine Morkovsky.

²³ Margry, *Découvertes et Établissements des Français*, 2: 199–200.

Mississippi divided like a hen's claw, he noted the natural embankments of silt stretching like levees or dikes along both sides of the channel and extending the riverbed "for more than six leagues into the sea." In scarcely two days' time, he prepared a very recognizable map of this unusual configuration²⁴ and correctly assessed that large ships could navigate the lower river basin (although for years the unique "mud lumps" and occasional "floating islands" that guard these passages made it problematical for sailors to find and enter the Mississippi from the sea).²⁵ Despite such accurate observations, La Salle had to assume, however mistakenly, that his long, southeasterly flowing river had brought him to the western edge of the gulf. He could not know that he had reached the tip of a vast alluvial peninsula that did not yet appear on any European map of the region.

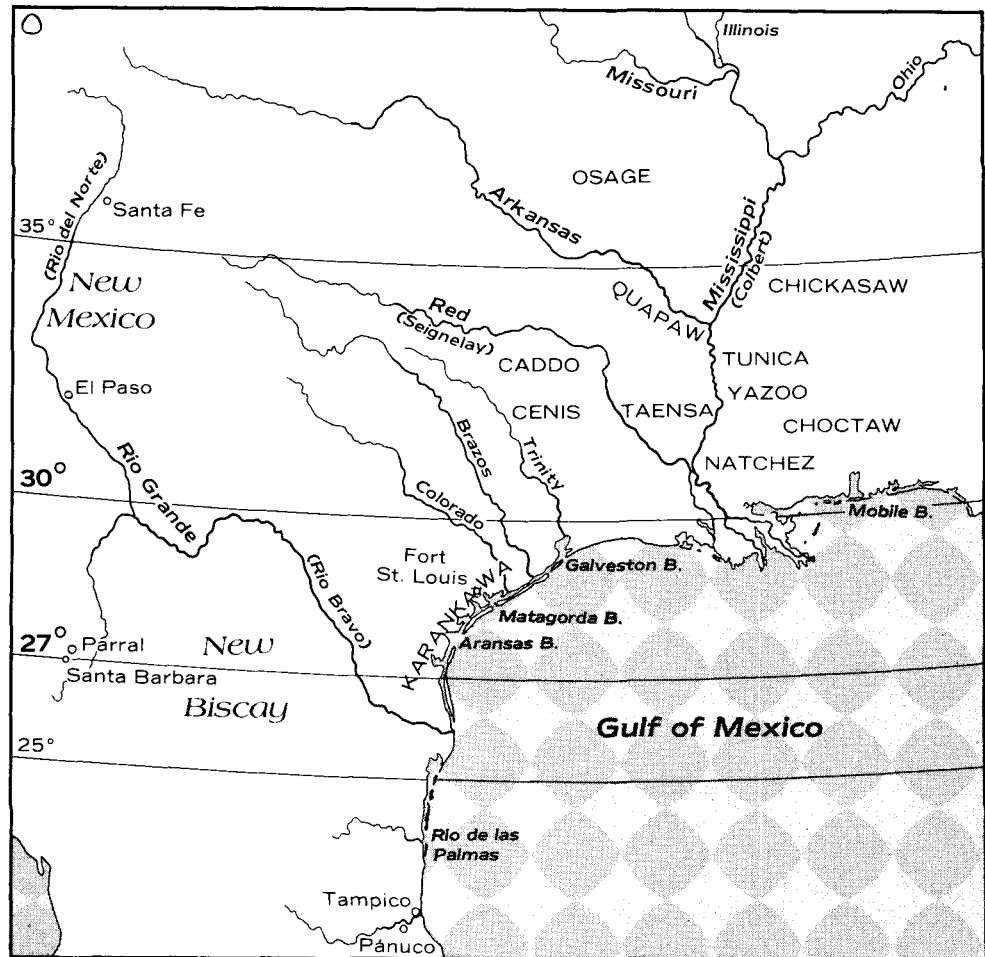
Up to this point La Salle's observations were unsettling and incomplete. Then, on April 8, according to the official *procès verbal*, the explorer "reascended the river, a little above its confluence with the sea, to find a dry place, beyond the reach of inundations. The elevation of the North Pole was here about twenty-seven degrees."²⁶ This latitude varies considerably from the correct figure of approximately 29 degrees 14 minutes and from some later reports that ranged between 27 and 29 degrees. The discrepancies sidestepped by Parkman and others call for an explanation, and many are conceivable. If La Salle wanted to extend his claim of first-hand discovery to the very border of New Biscay, perhaps he lied about the location (although believers in this deceit never explain why he returned in 1684–85 to a latitude he knew to be false). Or, if the explorer already suspected that he was on the Escondido, as Tonti recalled, then he could have "unconsciously" misread the astrolabe to reconcile the latitude with his own recent observations and with the Escondido's location on standard maps. The answer might even be, as several scholars have suggested, that "the figure 27°, which appears in the official relation, was inserted by someone other than the original author, in France."²⁷ The

²⁴ Joutel, as quoted in Delanglez, *Some La Salle Journeys*, 85; and Margry, *Découvertes et Établissements des Français*, 3: 28. For drawings by Minet and Franquelin based on La Salle's map, see R. Thomassy, *Géologie pratique de la Louisiane* (New Orleans, 1860), between pp. 16 and 17; and Chabot, "Journal inédit relatant les expéditions de Cavelier de La Salle," 9. For additional maps of the delta's evolution to the edge of the continental shelf, see John D. Ware and Robert R. Rea, *George Gauld, Surveyor and Cartographer of the Gulf Coast* (Gainesville, Fla., 1982); and Douglas Lee, "The Mississippi Delta," *National Geographic*, 164 (1983): 226–53. The Mississippi River carries over two million tons of soil to the gulf every day, adding another square mile every sixteen years to a delta that extends over twelve thousand square miles. "It appears certain," Pierre de Charlevoix observed in 1722, "that when M. de Sale went down the Mississippi to the sea, the mouth of this river was quite different from what it is at present"; Charles E. O'Neill, ed., *Charlevoix's Louisiana: Selections from the History and the Journal* (Baton Rouge, La., 1977), 171.

²⁵ Margry, *Découvertes et Établissements des Français*, 4: 51–57, 159. On mud lumps, see the diagrams in Thomassy, *Géologie pratique de la Louisiane*, between pp. 46 and 47. Also see the fascinating essay of Richebourg Gaillard McWilliams, "Iberville at the Birdfoot Subdelta: Final Discovery of the Mississippi River," in John Francis McDermott, ed., *Frenchmen and French Ways in the Mississippi Valley* (Urbana, 1969), 127–40. On the rare floating islands of swamp vegetation, dislodged by flooding, see the 1694 map in Cumming *et al.*, *Exploration of North America*, 151; and Edwin Way Teale, *North with the Spring* (New York, 1951), 76–82.

²⁶ Sparks, *La Salle*, 199.

²⁷ Osler, *La Salle*, 257. Also see Jean Delanglez, "La Salle's Expedition of 1682," *Mid-America*, 22 (1940): 19–21, 33. The *procès verbal* and scores of related manuscripts and maps recently appeared in a French tricentennial exhibit commemorating La Salle's discoveries. The illustrated catalogue prepared by the Archives de France, *Naissance de la Louisiane* (Paris, 1982), includes pictures of early modern, portable instruments for measuring latitude, but whether La Salle carried an actual astrolabe for this purpose or some type of quadrant or backstaff remains uncertain.



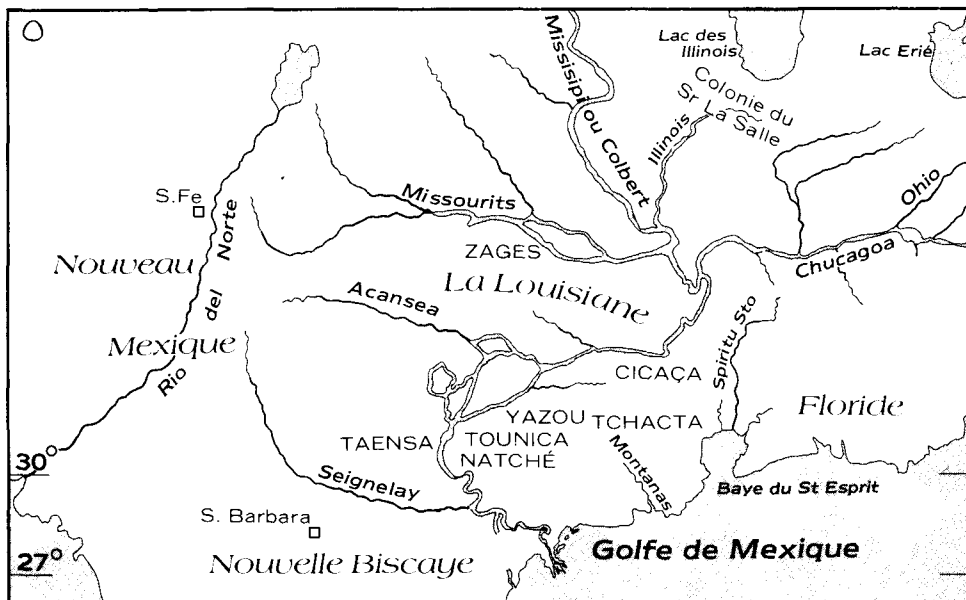
Map 3: The Lower Mississippi River Valley and related regions at the time of La Salle's explorations.
Map prepared by Robert L. Williams.

engineer Minet, who accompanied La Salle briefly in 1684–85 and transcribed his map of the lower Mississippi, offered a simpler explanation that suits the evidence without invoking complex conspiracies or mental machinations. He believed, perhaps on the basis of some direct knowledge, that the explorer's astrolabe was defective when he reached the gulf.²⁸

Did La Salle worry that he could be using a damaged or faulty astrolabe and therefore slightly hedge his findings after meeting with Bernou and others in 1684? Or did he scarcely suspect a misreading of latitude until the mistake came back to haunt him on his final journey three years later? Probably we shall never know the full story, but it is important to recognize the crucial error and its consequences. At the same time, it is significant to note that even a correct reading very near 29 degrees would have been disconcerting, since cartographers placed the north coast of the gulf, where La Salle expected to arrive, at 30 degrees. The whole startling turn of events raised enormous new geographical problems and possibilities, and it inevitably prompted concern and caution. La Salle's reading of his astrolabe,

²⁸ Minet to Seignelay, July 6, 1685, in Margry, *Découvertes et Établissements des Français*, 2: 603: "Sa hauteur n'est pas juste, n'ayant pas d'instrument bon pour la prendre." Minet recorded that near the mouth of the river La Salle took readings (plural) of the altitude and found it to be 28 degrees 20 minutes; Minet, "Voilage fait du Canada," f. 41.

whether broken or not, suggested that the expedition had somehow gone beyond its projected destination to a place where, according to gulf charts developed over more than one hundred and fifty years, no cape even existed.²⁹ Sensing that he was far from his expected destination and perhaps dangerously, though excitingly, close to New Spain, La Salle apparently chose to disguise the puzzling reading from his men, somewhat as Columbus had done on his first Atlantic voyage. He started back upstream immediately, pondering the strange direction of his river and the surprising, if uncertain, latitude of its mouth.



Map 4: The Lower Mississippi River Valley and related regions as understood by La Salle after his 1682 descent to the gulf. This outline sketch is based on copies of Franquelin's detailed map, now missing, which was presented to officials in Paris in 1684. Map prepared by Robert L. Williams.

OVER THE NEXT EIGHTEEN MONTHS La Salle engaged in the agonizing—but honest—task of trying to reconcile his key observations with each other and with European geographical knowledge, never entirely certain when his own reckonings or those of respected mapmakers might be at fault. Fortunately, we have a long segment of a letter that La Salle wrote in this period, which shows him struggling to resolve conflicting evidence and to determine exactly where he had been. The letter has been called the “Chucagua fragment” because in it the author puzzled over whether his Colbert or Mississippi was the same as the Chucagua River that Hernando de Soto’s Spanish forces had crossed more than a century earlier in an effort to reach Mexico. (Compare Maps 3 and 4.) This undated document has not received sufficient scholarly attention, in part because historians have been unable to

²⁹ Monique Pelletier, conservator of the map division at the Bibliothèque Nationale, recently illustrated the eventual mapping of the Mississippi and its delta by Guillaume Delisle and his son Claude at the turn of the century. She cited the instructive letter of Claude Delisle, regarding the disputed location of the river’s mouth, found in *Journal des sçavans*, May 17, 1700, pp. 362–73. Pelletier, “Exploration and Colonisation of Louisiana,” *Map Collector*, no. 23 (1983): 10–14.

perceive and take seriously on the explorer's own terms the enormous geographical dilemma with which he was wrestling.³⁰

Although it may appear far-fetched to us, La Salle had to begin his analysis by reconsidering whether his river somehow flowed into the Atlantic.³¹ He concluded it did not, but the idea was plausible, given the gross uncertainties about longitude and declination and the existence of French maps that showed Florida and the Virginia coast displaced far to the west in relation to Canada and the Great Lakes.³² Even now, after his descent, La Salle remained confident that the northern Gulf Coast was "almost everywhere at the 30th degree," as shown on most maps. So in trying to understand his data, he had to wonder whether the river ran much farther toward the southeast than he had estimated, sloping down across the Florida peninsula and exiting eastward into the sea at 27 degrees (in the vicinity, say, of modern-day Palm Beach). Although such a theory might reconcile his observations with existing cartography, it seemed to him implausible.³³

Hence La Salle turned to the other possibility raised by his observations: that the lower reaches of the river edged toward the southwest and then curved back to enter the western rim of the gulf beyond and below the Bay of Espíritu Santo, where maps showed the rivers Magdalena, de Pescadores, Escondido, Bravo, de Palmas, and Pánuco. This reconstruction explained the morning fogs that he had seen blowing in from what is actually Lake Ponchartrain and from the gulf east of the delta. "They were mists from the shore of the Bay of Espíritu Santo," La Salle reasoned, "and from the sea coast lying northeast-southwest from Escondido to de Pescadores and the above-mentioned Bay." His appraisal also fit the river's apparent southerly latitude and its observed flow toward the east-southeast. "Now this is precisely the direction of the Rio Escondido," he wrote. "For this reason I

³⁰ The letter referring to the Chucagua (or Choucagoua, or Chucagoa) may have been sent directly to Bernou or turned over to him eventually, for the existing fragment, without address or signature, is in Bernou's papers in Paris at the Bibliothèque Nationale, Collection Clairambault, vol. 1016, ff. 188–89v. For critical comments on the way this item was edited by Margry, translated by Steward, and interpreted by de Villiers, see Delanglez, *El Rio del Espíritu Santo*, 104 n. 22. Delanglez summarized the document and correctly linked it to the Franquelin chart of 1684 (outlined here in Map 4), but he failed to consolidate this evidence into his analysis. Likewise Osler, who seems to have followed Delanglez, overlooked the implications of the fragment and did not even give a citation for his comments on it: *La Salle*, 160. Galloway did not discuss the document in her otherwise excellent article, "Sources for the La Salle Expedition."

³¹ A decade earlier Marquette had wondered whether the Mississippi ended up "to the east in Virginia"; de Vorse, "Impact of the La Salle Expedition," 65.

³² For example, see Randin's map cited in note 18, above. The lack of firm knowledge regarding declination (the variation between magnetic or compass north and true north) was a major constraint. Scientist Edmond Halley, who had been collecting data on magnetic variation for years, did not publish the first isogonic chart (a world map showing lines of equal magnetic variation) until 1702. See Cumming *et al.*, *Exploration of North America*, 22.

³³ Margry, *Découvertes et Établissements de Français*, 2: 198; and Steward, "La Salle a Victim to His Error in Longitude," 130. After all, "the Colbert River would have no room in the east-west width of this peninsula," La Salle observed, "since it runs to the southeast for at least 120 leagues, from the latitude 30° to latitude 27°, at which point it empties into the sea. This would be impossible in the width of the Florida peninsula." Margry, *Découvertes et Établissements des Français*, 2: 198; and Delanglez, *El Rio del Espíritu Santo*, 105. If necessarily uncertain about the lower Mississippi's general location, La Salle was impressively clear about its specific shape, as can be seen by comparing his river outline from the Taensa villages to the gulf with modern landform maps. Copies based on his initial sketch clearly portray the entrance to Bayou la Fourche and the right-angle bend below what is now New Orleans.

maintain that we were near Mexico, and consequently in a river other than the Chucagua from which the Spaniards took such a long time to reach Mexico.”³⁴

The lopsided pattern of tributaries that La Salle observed on the lower Mississippi also contributed to his conclusion that “[the] Chucagua is different from [the] Mississippi and . . . goes along side by side with it.” The uneven distribution of entering streams suggested that the Mississippi lay west, not east, of the row of mountains that on traditional maps extended southward toward the northwest corner of the gulf. For La Salle had noted that “from the West some very large” rivers emptied into the lower Mississippi, and he gathered that the Indians living near these streams were familiar with Spanish trade goods and hated the Spaniards for trying to enslave them. The explorer concluded that the western tributaries would not be so large and their inhabitants would not have so much contact with the Spanish if the Mississippi flowed east of the supposed mountain range. But if the river descended just to the west of such a chain of hills, he thought, then that would explain his observation—which was accurate—that “from the East of the Mississippi no considerable” rivers discharge into its lower course.³⁵ If in fact there was another river further east, and the Mississippi was not de Soto’s Chucagua, this would be significant for La Salle: it would mean the Indians were correct in telling him that he was the first European on his river’s lower segment and that he was justified in claiming it for France against other Europeans by precedence of discovery.

La Salle did not write frequently or easily,³⁶ and the awkward prose of the “Chucagua fragment” shows him struggling to relate his own experience to de Soto’s findings and to reconcile his first-hand observations with the maps that he studied before setting out. Because the river he explored had large tributaries on the west and not on the east, because it appeared to extend several degrees below “the southern coast,” and “because its mouths [were] to the East-Southeast and not to the South,” La Salle believed his river reached the portion of coastline “which runs from the river called in the maps Escondido to the Panuco.” He was trying to consider and expand European knowledge rather than to dismiss or distort it, and

³⁴ Margry, *Découvertes et Établissements des Français*, 2: 197–200. La Salle wrote, “assuredly the account of Fernand Soto is not a chimera.” Therefore he worked carefully in the “Chacagua fragment” to reconcile his observations with the Spaniard’s. But La Salle was struck by the fact that de Soto’s tribes had different names from those that the French expedition found, and de Soto’s Indians were more numerous. Not knowing that foreign diseases had been reducing the indigenous population along southern coasts and riverine trade routes for more than a century, La Salle thought his predecessor’s findings unlikely. Because he observed fewer people than de Soto and witnessed extensive April flooding, La Salle concluded mistakenly that “the banks of the Mississippi” had never been “in a condition to be more populous . . . because nearly all that is not inhabited is subject to almost continual inundation”; *ibid.* Also see the suggestive new arguments of Henry F. Dobyns, *Their Number Become Thinned: Native American Population Dynamics in Eastern North America* (Knoxville, Tenn., 1983), 250–74, 308.

³⁵ La Salle recorded, “I have always conjectured there must be, to the East some other great river where all of the waters of that side are emptied. In truth, when one has traveled a day or two in the woods [east of the lower Mississippi] all the streams and rivers which one finds descend towards the East and not one towards the Mississippi.” Margry, *Découvertes et Établissements des Français*, 2: 199; and Steward, “La Salle a Victim to His Error in Longitude,” 131.

³⁶ See Parkman, *La Salle*, 339–40. William Taylor pointed out that Parkman “considerably improved the literary quality—and, in fact, the literacy—of La Salle’s letters in the translations he made of them from French documentary sources”; Taylor, “Journey into the Human Mind,” 231.

his labors led inexorably to the conclusion, "This Escondido is surely the Mississippi."³⁷ He then prepared a map embodying the convictions expressed in the "Chucagua fragment." He concluded that the lower Mississippi, instead of dropping directly into the Bay of St. Esprit at 31 degrees, as geographers had predicted, must stretch eight or nine degrees westward and then curve back into the gulf near 27 degrees. The explorer shared his map with Jean-Baptiste Louis Franquelin, Canada's foremost cartographer, who copied and refined it. Apparently, Franquelin traveled to France with La Salle when the explorer, after long delays, finally crossed the Atlantic at the end of 1683.³⁸

Just as we have hints of how La Salle made sense out of his travels, we also have bits of evidence showing how word of his journey down river and its interpretation—a highly sensitive piece of international intelligence—filtered back to interested parties in Quebec and Paris before his arrival. In the early summer of 1682, when a serious illness halted La Salle's return upstream for more than a month, Tonti went ahead to Michilimackinac and provided supporters with the first news of where the expedition had been and what they believed they had discovered. "We have been below the 29th degree," he wrote in late July, "leaving the Baye du St. Esprit on our left to the northeast. M. de la Salle believes the mountains of St. Barbara are eighty leagues from there. He has kept to himself the [exact] latitude of the mouth. We traveled south and southwestward." La Salle recovered enough to reach Michilimackinac in the fall. In October he wrote to a benefactor, "Though my discovery is made, and I have descended the Mississippi to the Gulf of Mexico, at 27 degrees north latitude, it is impossible for me to send you this year either an account of my journey or a map."³⁹

Now, as at earlier times in his life, La Salle saw himself beset by menacing forces: bodily infirmity, financial creditors, and political rivals. From Quebec came word that Count Frontenac, who had always looked out for the explorer's interests, had been replaced as governor of Canada by the hostile Monsieur La Barre. Beyond these adversities loomed another pressing danger; the powerful Iroquois were again threatening to attack the Illinois country where La Salle had been cultivating their longtime enemies as potential participants in his inland trading empire.⁴⁰ The explorer needed to go east to mend fences and consolidate the geographical and

³⁷ Margry, *Découvertes et Établissements des Français*, 2: 198. If anything, La Salle took the crude, conventional maps of the day too seriously, and he was pushed southwest in his focus by logic rather than by greed. Either "the mouth of the Colbert River is near Mexico," he wrote emphatically, or "all the maps are worth nothing"; *ibid.* On La Salle's mapmaking, see Delanglez, *Some La Salle Journeys*, 35.

³⁸ Jean Delanglez, "Franquelin, Mapmaker," *Mid-America*, 25 (1943): 59–60. Delanglez called the cartographer's map of La Salle's travels (finished in 1684 and now lost) "the best known of Franquelin's maps." A nineteenth-century copy of the map, made for Parkman and deposited in the Harvard Library, can no longer be found there. Fortunately, Reuben E. Thwaites reproduced the Parkman facsimile as the frontispiece to his *The Jesuit Relations and Allied Documents*, 63 (Cleveland, 1900).

³⁹ Habig, *Franciscan Père Marquette*, 229; and Parkman, *La Salle*, 311. Sketching briefly the prospects for controlling the river and developing the interior, La Salle noted, "The usefulness of this enterprise seems to lie primarily in the convenience of the harbors that the mouths of the river form in the vicinity of the Spaniards and close by the routes taken by their fleets, where it will be easy to sustain a colony because of the fertility and goodness of the land. . . . The river's mouth is easy to defend." Margry, *Découvertes et Établissements des Français*, 2: 292.

⁴⁰ See Osler, *La Salle*, 153–56. For a recent summary of the political forces at work, see W. J. Eccles, "The Fur Trade and Eighteenth-Century Imperialism," *WMQ*, 40 (1983): 341–45.

economic implications of his adventure, but he first had to return to the Illinois River, where he joined Tonti during 1683 in constructing Fort St. Louis and in encouraging numerous Indian allies to settle near what Parkman called his “singular extemporized colony.” As he told his patron in the letter from Michilimackinac, “my presence is absolutely necessary in the place to which I am going.”⁴¹

Late in 1682, while La Salle made his way to the Illinois, the Franciscan who had nursed him back to health the previous summer took an account of the expedition to France. Father Membré sailed from Quebec in mid-November on the same ship with Count Frontenac, and he presumably carried his own journal and the *procès verbal* attested at the mouth of the Mississippi. In Paris early in 1683, letters written the previous year by Membré, Tonti, and La Salle were combined into an official version, *Relation of the Discovery*, which shows how La Salle’s European supporters accepted and developed his general interpretation of where he had been. The editor was Abbé Bernou, an expert on the existing cartography of the gulf region who had a keen interest in La Salle’s exploration. He incorporated into the report his own statement regarding the apparent southwestern location of the lower Mississippi. “It descends into the Gulf of Mexico beyond the bay of Saint-Esprit,” he observed, “between the 27th and the 28th degree of latitude, and at the place where some maps indicate the *Rio de la Madelana*, and others the *Rio Escondido*.”⁴² These two rivers were one on Pierre du Val’s chart of “La Floride” (Map 1; see page 300, above) that had been republished the previous year, so Bernou may have had this sketch map by the king’s geographer before him as he wrote.⁴³

Louis XIV either did not see or did not accept this initial summary of La Salle’s expedition. In response to a skeptical letter from the new governor of Canada, the king replied, “I am persuaded, with you, that Sieur de la Salle’s discovery is very useless.” But in October Spain declared war on France in retaliation for Louis XIV’s aggressive policies in Flanders. So when La Salle, accompanied by Nicholas and Barbier who had shared the river journey, reached Paris several months later with detailed new charts showing the apparent course of his river, official receptivity began to shift. “He found a great welcome with Monsieur de Seygnalay, to whom he presented a map marked with latitudes and longitudes that would make plausible his proposals,” reported a doubting Spaniard several years later. “On his

⁴¹ Parkman, *La Salle*, 316 n. 1, 311–12. “He meant,” Parkman explained, “to found on the banks of the Illinois a colony of French and Indians to answer the double purpose of a bulwark against the Iroquois and a place of storage for the furs of all the western tribes; and he hoped in the following year to secure an outlet for this colony and for all the trade of the valley of the Mississippi, by occupying the mouth of that river with a fort and another colony”; *ibid.*, 312.

⁴² Thomassy, *Géologie pratique de la Louisiane*, 14. Bernou added that La Salle, “who always carries an astrolabe on his voyages, has taken the precise latitude of this river mouth”; *ibid.*, 15. For a discussion of Bernou’s authorship of this official relation, see Jean Delanglez, “La Salle’s Expedition of 1682,” 3–37.

⁴³ If the mouth of La Salle’s river coincided with that of the “Escondido or Madalena” on du Val’s map, then it lay southwest “about 30 leagues from *Rio Bravo*” and northeast “about 60 leagues from *Rio de Palmas*, and about 90 to 100 leagues from *Rio Panero* [Pánuco] where the nearest settlement of Spaniards is located along the coast”; Thomassy, *Géologie pratique de la Louisiane*, 14–15. Delanglez linked Bernou’s statement to du Val’s map (apparently following de Villiers, *L’Expédition de Cavelier de La Salle*, 33), but he mistakenly interpreted Bernou’s words to mean that the *Rio Bravo* was “30 leagues north of *Rio de Palmas*” and therefore 30 leagues south of the *Madalena* or *Escondido*. Delanglez also stated that “the French counted 25 leagues to the degree,” whereas on most maps twenty French leagues equal one degree of latitude. Delanglez, “La Salle’s Expedition of 1682,” 20.

word they believed in him and his map. . . . He convinced everyone except the Jesuits (whom he held as enemies)." One of the people the explorer convinced was Abbé Tronson, the superior of the St. Sulpice Seminary, who later wrote, "I had a long conversation with M. de la Salle regarding his discovery, of which he gave me a very beautiful map." The Paris seminarian recorded, "he contends he entered the Gulf of Mexico not by way of the Baye du St. Esprit, but at latitude 27°, on the same meridian as Panuco, which is at the end of the Gulf, far beyond this bay." By late March La Salle had also convinced Louis XIV, and in April the king granted him supreme command "from Fort St. Louis on the Illinois River to New Biscay."⁴⁴

DID A BOLD SCHEME TO COLONIZE THE MISSISSIPPI and to attack New Spain give rise to a willful distortion of geography, or did a plausible geographical error pave the way for an elaborate imperial design? The scholarly answer to this question has shifted over the years. Parkman believed that the plan embodied in the successful memorials prepared for the crown early in 1684 was "based on a geographical blunder."⁴⁵ But instead of analyzing La Salle's observations and his subsequent dilemma, Parkman chose to hint at flaws in the explorer's character that later historians have grossly inflated. "What Parkman termed a blunder is in reality a deception," declared the Jesuit scholar Jean Delanglez in 1938. "La Salle adapted the geography of the river he had explored in order to secure financial help," he charged. "When it suited his purposes to deceive people, he was not one to be bothered by scruples." And what Delanglez termed "a deception" Louis de Vorsey, Jr., has recently called "the greatest geographical hoax in the history of North American exploration." For present-day historians the expansive and partisan memorials of 1684, clearly written by publicists to promote their project, now appear as no more than "a series of lies and deceptions, misrepresentations, and fantasies." Scholars have argued that king, counselors, and cartographers alike were "duped by La Salle into believing that the Mississippi River found its way to

⁴⁴ Louis XIV to La Barre, August 5, 1683, in E. B. O'Callaghan, ed., *Documents Relative to the Colonial History of the State of New-York*, 9 (Albany, 1855): 201; Walter J. O'Donnell, trans., "La Salle's Occupation of Texas," *Texas Catholic Historical Society Preliminary Studies*, 3 (Austin, 1936): 29–30; and Margry, *Découvertes et Établissements des Français*, 2: 355, 382–83. On February 7, 1687, Don Pedro Ronquillo, Spanish ambassador to England, wrote to the king of Spain summarizing all he had learned regarding La Salle as the Spanish finally began to gather intelligence regarding the French incursion. His patchwork report put the explorer's return to France at the end of 1684 rather than 1683, but he gave the crown valuable information. Ronquillo's account provides historians with one version of La Salle's enterprise that was beginning to circulate in European courts by the year of the explorer's death. Ronquillo told the king that La Salle's Mississippi "empties into the Gulf of Mexico between the Magdalena River and Espiritu Santo Bay"; O'Donnell, "La Salle's Occupation of Texas," 29–30. Recently, Pelletier noted—in line with the argument developed here—that "La Salle, who had estimated the mouth of the Mississippi to be at 27° latitude, had concluded from this that he was at the height of the river Escondido, far from this Bay of the Holy Spirit but close to the Rio Bravo and . . . New Biscay"; Pelletier, "Exploration and Colonisation of Louisiana," 12. (Pelletier's article appeared after this essay was completed.)

⁴⁵ Parkman, *La Salle*, 348. The nature of this blunder, Parkman continued, "is explained by the map of La Salle's discoveries made in this very year [1684]. Here the river Seignelay, or Red River, is represented as running parallel to the northern border of Mexico, and at no great distance from it—the region now called Texas being almost entirely suppressed. According to the map, New Biscay might be reached from this river in a few days; and, after crossing the intervening forests, the coveted mines of Ste. Barbe, or Santa Barbara, would be within striking distance. That La Salle believed in the possibility of invading the Spanish province of New Biscay from the Red River there can be no doubt." *Ibid.* See La Salle's letter of October 1682, in Margry, *Découvertes et Établissements des Français*, 2: 293.

the sea in Texas, more than 600 miles from its actual mouth in Louisiana” and that this self-serving design was capped “by simply uniting the Rio Grande and the Mississippi.”⁴⁶

To scholars secure in their knowledge of the modern identity and actual location of these two rivers, any theory linking the Mississippi to the Rio Grande seems absurd. But in fact this unlikely combination hinged on a breakthrough in geographical understanding provided by the exiled former governor of Spanish New Mexico, Count Peñalosa, who came to France in the 1670s. A map in the Naval Library in Paris (ca. 1680) indicates that Peñalosa, on the basis of first-hand knowledge, enlightened French cartographers that the Rio Grande (or what the Spanish usually called the Rio del Norte and then, lower down, the Rio Bravo) does *not* flow into the remote Gulf of California, as had been traditionally assumed.⁴⁷ Instead, after descending south past Santa Fe to El Paso, it turns east—not west—and empties into the Gulf of Mexico near 27 degrees. This meant that the vague entrance to the “Rio Bravo”—marked on French maps of the Gulf Coast either just north or just south of the Escondido—was in fact a mighty river that reached all the way to the rich province of New Mexico. In 1682 Abbé Bernou, advised by Peñalosa, described the river’s course, and later the Venetian cartographer Coronelli correctly placed the Rio Bravo on the globe he prepared for Louis XIV.⁴⁸

The strategic significance of this breakthrough was obvious, and an expedition by sea against the Spanish at the western end of the gulf had been discussed by the French from the time Count Peñalosa arrived in Paris. Working through Bernou, Peñalosa proposed to establish a colony north of Pánuco, probably “at the mouth of the Rio Bravo,” and move inland to discover the fabled kingdom of Quivira or to conquer the mines of New Biscay.⁴⁹ If La Salle had really reached the mouth of the

⁴⁶ Delanglez, *Some La Salle Journeys*, 79–82; de Vorsey, “Impact of the La Salle Expedition of 1682,” 70; Folmer, *Franco-Spanish Rivalry*, 144; and de Vorsey, “Impact of the La Salle Expedition of 1682,” 73. Delanglez stated that if La Salle “did not actually write the memoirs . . . incorporating false geography with malice aforethought, to deceive Seignelay, he countenanced the deception and thus he certainly cannot be held innocent of willful falsifications with regard to the maps.” For “the maps made, under his direction after his return to France in 1684, show a deliberate tampering with the geography of the Lower Mississippi such as can be explained only by his will to deceive Seignelay by making the river flow near New Biscay.” Delanglez, *Some La Salle Journeys*, 80, 79.

⁴⁷ Carl I. Wheat, *The Mapping of the Trans-Mississippi West, 1540–1861*, 1 (San Francisco, 1957); plate 61. Also see Henry R. Wagner, *The Spanish Southwest, 1542–1794: An Annotated Bibliography* (Albuquerque, N.M., 1937), 282–83.

⁴⁸ Delanglez, *Some La Salle Journeys*, 67; and de Vorsey, “Impact of the La Salle Expedition of 1682,” 71–72, and fig. e. In 1682, Bernou proposed to Seignelay the establishment of a colony “at the mouth of the river called Rio Bravo.” He reported, “This great river has its origin to the north of New Mexico . . . under the name of the River of the North. It has a course of more than 500 leagues, and it flows from north to south until it reaches a settlement of the Spaniards called the Passo [El Paso], where it turns toward the east, and after having received a great number of rivers, it empties into the Gulf of Mexico at 27° latitude.” Delanglez, *Some La Salle Journeys*, 67 n. 10. Dunn stated that, in 1678, when the Spanish Council confronted the matter of Peñalosa’s defection to France and the threat posed by his knowledge of New Spain, the council suspected that he “might do much harm to the interests of Spain if allowed to remain among her enemies, and urged that some means should be devised for spiriting him away from France”; Dunn, *Spanish and French Rivalry*, 16. Pelletier reproduced the “large and beautiful map” of North and Central America prepared before 1682 at Bernou’s direction, which incorporated current French knowledge of the upper Mississippi and the Mexican borderlands and left the intervening space bare in anticipation of La Salle’s descent to the gulf; Pelletier, “Les Globes de Louis XIV: Les Sources françaises de l’œuvre de Coronelli,” *Imago Mundi*, 34 (1982): 72–89. Also see Tucker, *Indian Villages of the Illinois Country*, 4 and plate viii; and “Carte de l’Amérique Septentrionale,” cover illustration of this issue.

⁴⁹ Delanglez, *Some La Salle Journeys*, 66 n. 7. Also see Folmer, *Franco-Spanish Rivalry*, 137–44.

Mississippi and it lay where his calculations suggested it must, then a new set of plans might be prepared. La Salle met with Peñalosa at least once, and in January 1684 a memorial was drawn up to arouse government interest. (The exact authorship is unclear.) “The Spaniard’s having just declared war on his Majesty,” the document begins, “he seems to be fully justified in employing the great means which Providence affords him of profiting by so rash a declaration.” The memorial first repeats Peñalosa’s design for a direct coastal invasion of New Spain and then outlines La Salle’s overlapping scheme, beginning with the startling conjecture that the Rio Bravo (or Rio Grande) and the Mississippi were one.⁵⁰

Such a speculation was by no means far-fetched. For if La Salle’s Mississippi was indeed the Escondido and reached the gulf near 27 degrees, as he calculated, then it seemed to Bernou and others that “the mouth of the Mississippi cannot be far from that of the Rio Bravo.” Indeed, since the lower Rio Bravo was now understood to have a similar size, latitude, and direction as the lower Mississippi, could they in fact, the French wondered, be the same river? Perhaps, they reasoned, when La Salle had reached the large stream flowing from the west that he called the Seignelay, he had descended into what was actually the Rio Bravo.⁵¹ Whether the lower Mississippi turned out to be the lower Rio Bravo or some neighboring river, the February memorial to Seignelay suggested that with prompt backing La Salle could begin the following “winter to spread terror in that part of New Biscay which lies in the vicinity of the river he has discovered.”⁵²

Historians who have dismissed the cartography that put La Salle’s river near Mexico have also found ludicrous the claims in the 1684 memorials that the explorer had prospective Indian allies for an assault on New Spain. Modern writers, who forget that La Salle’s ability to befriend and mobilize Indians was perhaps his most consistent strength,⁵³ charge that he inflated their numbers, dissembled about their motivation, and exaggerated their potential effectiveness. Yet, unlike his geographical appraisals, La Salle’s first-hand observations of Indians

⁵⁰ Margry, *Découvertes et Établissements des Français*, 3: 48–63; and Delanglez, *Some La Salle Journeys*, 71. The memorial continued, “The other means to bring about the successful conquest of New Biscay is to ascend the river called by the Spaniards Rio Bravo, which luckily happens to be the same as that which the savages call Mississippi, which Sieur de la Salle, governor for the king of Fort Frontenac, in New France, has just explored down to the sea and has arrived recently in Paris to give an account of his discovery”; Delanglez, *Some La Salle Journeys*, 72.

⁵¹ Bernou to Renaudot, January 25, 1684, in Delanglez, *Some La Salle Journeys*, 70. Bernou wrote Renaudot that La Salle’s river appeared to provide “a link between New Biscay and Canada.” He added, “I am confirmed in this by what M. de Villermont writes me, namely, that M. de La Salle told him that he had found a river coming from the West, of a length of thirty days march, which flows in the Mississippi 40 or 60 leagues from the sea. If it should happen to be the Rio Bravo, it would supply a very safe and very easy means of communication with New Leon, New Biscay, and New Mexico. Should the Peñalosa expedition be delayed, he will all the more be able to persuade the Ministers of the necessity of his going ahead to begin his settlement and of learning all that is necessary. With this information everything else can be undertaken without fear of making any mistake.” Bernou to Renaudot, February 29, 1684, in Delanglez, *Some La Salle Journeys*, 77–78.

⁵² Margry, *Découvertes et Établissements des Français*, 3: 69. In the same month a naval captain from St. Augustine sought permission to explore the northern Gulf Coast for Spain. Martín de Echagaray proposed to locate the Bay of Espíritu Santo into which—he heard from the Indians—emptied two large rivers, one flowing south through “Movila” and the other bending to the west toward the region of New Mexico. Dunn, *Spanish and French Rivalry*, 20, 22, and map, p. 44.

⁵³ Regarding the 1682 exploration, Galloway concluded, “Probably the most important accomplishment of La Salle’s journey was the establishment of peaceful relations with the Native American groups the party met along the way”; Galloway, “Introduction,” in Galloway, *La Salle and His Legacy*, ix–x.

in the Mississippi Valley were undoubtedly better, not worse, than the uninformed dismissals offered by colonial historians in their standard accounts. For example, several years after the successful Pueblo Revolt, which pushed the Spanish south out of New Mexico in 1680, La Salle saw its impact in the rapid spread of horses, and of anti-Spanish feeling, among the peoples of the trans-Mississippi west. It scarcely matters whether La Salle had personally mastered ten Indian languages as some of his supporters claimed, for on the basis of experience he felt confident of his ability to converse with at least that many tribes through translators. And he had sound appraisals of the number of possible Indian allies through informants and personal observation.⁵⁴ From the time of Cortez, a nucleus of European soldiers often joined with a larger force of Indian allies to conduct a military campaign in the New World; the French in particular had learned to take advantage of regional animosities and to seek support from disaffected subjects within the enemy's domain.⁵⁵ Given these French assumptions of imperial warfare, and given conditions on the frontier of New Spain, the grandiose plan of 1684 was more predictable than far-fetched, and it foundered on geographic, not ethnographic, misapprehensions.

If, at the time, there was some semblance of internal logic in the double-edged plan to colonize the Mississippi and to invade nearby Mexico, why was La Salle supposedly indecisive and secretive following its approval? "Now," according to a traditional scenario, "burdened down by a crushing awareness of the impossible position in which he had placed himself with his dishonest proposal to the King, his precariously balanced reason had become overthrown and he had begun transferring his own self-doubts to Beaujeu, Arnoul, . . . and anyone else who dared to question the wisdom of his erratic course."⁵⁶ In fact, the hesitations and secrets of

⁵⁴ Conventional American historians still know too little about matters of trade, travel, communication, and demography among seventeenth-century Indian nations, but see Elizabeth A. H. John, *Storms Brewed in Other Men's Worlds: The Confrontation of Indians, Spanish, and French in the Southwest, 1540–1794* (College Station, Tex., 1975), 112; and Richard White, *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos* (Lincoln, Nebr., 1983), 153–54. White put the population of the Pawnees, potential allies of La Salle, at twenty thousand. According to W. David Baird, "Father Membré calculated the Quapaw population as 15,000 to 20,000 souls. His estimate was probably an exaggeration, for Tonti later estimated the warriors as numbering no more than 1,500"; Baird, *The Quapaw Indians: A History of the Downstream People* (Norman, Okla., 1980), 23. According to La Salle's own village-by-village enumeration, the explorer had access to some four thousand warriors along the Illinois. He had carefully established good relations with the larger nations farther south, and he had sound intelligence of potential allies to the west. Therefore, his projected force of fifteen thousand, while optimistic, was by no means fantastic. Margry, *Découvertes et Établissements des Français*, 3: 55–63. Several years later the Spanish discovered a single French soldier close to the Rio Grande who had more than one thousand Indian bowmen at his command. On this supposedly "demented Frenchman" who has not yet been fully studied, see Weddle, *Wilderness Manhunt*, 138–40.

⁵⁵ In 1697, Louvigny prepared a memorandum for Louis XIV in which he offered to harass the border of New Spain for France if the king provided him with one hundred men; Margry, *Découvertes et Établissements des Français*, 4: 9–18; and Tucker, *Indian Villages of the Illinois Country*, 6 and plate xiv. Two years later, when Iberville was at Biloxi, he encountered several men who had worked in Mexican mines, and they told him "a great deal about the weakness of the Spaniards in that country." He wrote, "It would be a fine thing to go there and get silver if we should go to war with them. Five hundred good Canadians would make that whole country tremble," and, with "little expenditure, many millions could be taken out." McWilliams, *Iberville's Gulf Journals*, 95. During the eighteenth century, the mines of Santa Barbara proved among the richest in the New World, yielding silver worth several hundred million dollars; O'Neill, *Charlevoix's Louisiana*, 180 n. 14.

⁵⁶ Osler, *La Salle*, 179. Beaujeu was the career officer assigned to transport La Salle's company to the gulf; Arnoul was the intendant in charge of the port of Rochefort, where they embarked.

the early summer involved more than the grating together of abrasive personalities. There was, first of all, the sheer press of time. La Salle considered delaying the venture until an armed vessel could reconnoiter the Gulf Coast and report on military and geographical uncertainties, but, since this would have taken a year to complete, he faced the possibility of losing both support in France and the element of surprise in the gulf. Spain, goaded to declare war in October 1683, now feared the prospect of French aggression in America, and time became crucial for La Salle if he was to combine his colonization plan with Peñalosa's scheme of a direct attack.⁵⁷ On March 4, 1684, the king's minister, the Marquis of Seignelay, sent orders to the French governor in Santo Domingo to mobilize buccaneers to take part in an attack on New Biscay in October, but the order was apparently rescinded weeks later as prospects improved for a summer end to hostilities.⁵⁸ It seems to have been the French government more than La Salle himself that vacillated with regard to the exact extent and nature of the planned aggression "beyond the line" in the New World.

As for secrecy, since La Salle received command of a massive covert operation during time of war, the information blackout so frustrating to court gossips and historians should present no surprise. La Salle must have worked assiduously to disguise his sudden change of fortune.⁵⁹ Bernou, fearing the intrusion of the Jesuit "Robes Noires," told Renaudot, "Keep the negotiations of our friend a strict secret until he has left, for no matter what the R. N. put on, they will never approve of it; he knows this as well as you." Even the king himself was cryptic, granting La Salle authority over "the French and Indians whom he will employ in the enterprises we have entrusted to his care" and over "the country which will be subject anew to our dominion in North America."⁶⁰ But outfitting four ships and recruiting several hundred soldiers and colonists could not be kept entirely quiet. In early summer Bernou wrote to Renaudot from Rome, "this affair is not as secret as you believe"; he had read the following in a Dutch broadside of May 23:

According to news received from Paris, the Sieur de la Salle has gone to America and has penetrated so far that it can be said he has discovered a new world in the New World, a country very rich especially in silver mines. He has rendered an account of his voyage to the king who has given him money and ships to return thither. But it is to be feared that this marvelous expedition might turn out to be . . . a myth . . . and that this great journey which is to be achieved on the word of one man come to nothing. What looks rather suspicious is that the map of the country he visited is kept so well concealed.⁶¹

Only by concealing the map could the French authorities hide the apparent location of La Salle's "new world" on the gulf and take advantage of this seeming

⁵⁷ Delanglez, *Some La Salle Journeys*, 83. "For either the undertaking of the Count will take place before the peace (with Spain), or it will be postponed," Bernou wrote at the end of February; *ibid.*, 77.

⁵⁸ Margry, *Découvertes et Établissements des Français*, 2: 377. The so-called Peace of Ratisbon was not concluded until the Treaty of Tregua was signed at Regensburg on August 15, three weeks after La Salle sailed, but when he arrived at Santo Domingo and freebooters urged him to join them in an immediate attack on New Spain, he reportedly demurred, saying that "he had no order from the King of France." Weddle, *Wilderness Manhunt*, 218.

⁵⁹ "His affair is still very secret," wrote Abbé Tronson on April 10, 1684, "and there is no one in the world, up to the present, who does not believe it to be rejected"; Margry, *Découvertes et Établissements des Français*, 2: 354.

⁶⁰ Delanglez, *Some La Salle Journeys*, 76, 84; and Margry, *Découvertes et Établissements des Français*, 2: 383.

⁶¹ Delanglez, *Some La Salle Journeys*, 84–85. Also see Margry, *Découvertes et Établissements des Français*, 2: 355–56.

geographical windfall without threat of discovery or reprisal. Even Captain Beaujeu was not informed of their exact destination until the ships were underway, and he was irritated when La Salle told him that on arrival "it would be necessary to prevent any one from taking the latitude of the coasts."⁶² Like all good strategists, the French appeared to weigh up to the last moment the pros and cons of several approaches, trying to keep the alternatives open and the opposition confused. Once the existence of the expedition became undeniable, the French gave out broad hints that the ships were headed for Canada. Renaudot, writing in a mixture of arcane languages, disclosed to another supporter of La Salle in late April, "Thou must keep the secret in the bottom of thy heart, the captain of the ship himself does not know it, a secret decree has been rendered in the king's council, by which La Salle, crossing the Ocean (is directed to go) to the new river Mississippi and not to Canada."⁶³

Not until the moment of departure in July 1684 did La Salle finally inform his mother in Rouen, "We do not go by Canada at all but by the Gulf of Mexico."⁶⁴ European courtiers attentive to current affairs were kept successfully in the dark, however, and to complete the disguise the departing vessels sailed as far as Finisterre with a squadron of ships bound for Canada. As for La Salle's secrecy with his company about the latitude for which they sailed, his strict policy paid immediate dividends in Santo Domingo when several sailors jumped ship and joined a pirate crew later captured by the Spanish navy. Brought to Vera Cruz to be hanged in the fall of 1685, the deserters gave Spanish authorities their first inkling of La Salle's major incursion, but the captives could not state his exact destination even though their lives depended on it. According to the admirals who interrogated them, the renegade sailors knew only that, during La Salle's 1682 descent of the Mississippi, "about half a league away from the sea the altitude of the sun was taken, but the prisoners declared they did not know at what latitude they were; all that they know is that this [place] was called *Bahia de Misipipi*."⁶⁵

AFTER THREE HUNDRED YEARS we still do not know definitively the spot on the Texas coast where the company disembarked in late February 1685, much less the exact courses of La Salle and Beaujeu as they sailed separately along the shore for several

⁶² Sparks, *La Salle*, 118. Also see Margry, *Découvertes et Établissements des Français*, 2: 384. A quarter century later, Carolina adventurer Thomas Nairne, writing from jail in South Carolina, sent a strategic map of the South to London and pleaded for an English colony west of the Mississippi based on "La Salle's design formed against ye mine country." He, too, suggested an alliance with the "considerable numbers of Indians there . . . that are at constant wars with the Spaniards of new Mexico," and he added that "a thing of this nature must be done with great secrecy." Great Britain, Public Record Office, *Calendar of State Papers, Colonial Series: America and West Indies*, 19 (London, 1922): 424.

⁶³ Delanglez, *Some La Salle Journeys*, 86, n. 90. A safer, slower advance through Canada and down the Mississippi had much to recommend it, though Bernou, kept in the dark about the course selected, remained "convinced that the route through the Gulf is much better"; *ibid.*, 74. In 1697, Sieur Rémonville, an old acquaintance of La Salle, put forth two related plans for sending a force from Canada down the Mississippi and for approaching the river by sea via the gulf. He hoped to erect a fort near the mouth of the Mississippi and to secure the river valley for France. Margry, *Découvertes et Établissements des Français*, 4: 19–43.

⁶⁴ Margry, *Découvertes et Établissements des Français*, 2: 470; and Frances Gaither, *The Fatal River: The Life and Death of La Salle* (New York, 1931), 213. Nearly a year later the Spanish ambassador to England and others in London still believed that the French expedition had set out for Montreal, intending a laborious inland descent to the gulf from the north; Dunn, *Spanish and French Rivalry*, 52–53.

⁶⁵ Letter of Admiral Palacios, as quoted in Delanglez, *El Rio del Espíritu Santo*, 117. Also see Roberto Gil

weeks before that, looking for each other and for an entry to the Mississippi. Nevertheless, by setting aside our modern understanding of the gulf and taking up their limited cartography, we can make sense of their movements without resorting to purely psychological explanations. "Historians are still at loggerheads as to why the expedition, when it came into the Gulf of Mexico, overshot the mouth of the Mississippi and ended on the Texas coast."⁶⁶ Did La Salle miss his objective by mistake, or did he slide past it purposefully to get closer to New Spain? By now it should be obvious that neither of these standard alternatives hits the mark. If the explorer and his supporters believed that access both to the Mississippi and to the silver provinces of New Spain lay on the northwestern edge of the gulf, then we would expect La Salle to have headed directly to that portion of the coast. Given his prior calculations, he had no reason to look in the area where we now know the base of the river to be, even if pilots aboard argued from the start (as one survivor recalled they did) that he was searching too far west.⁶⁷

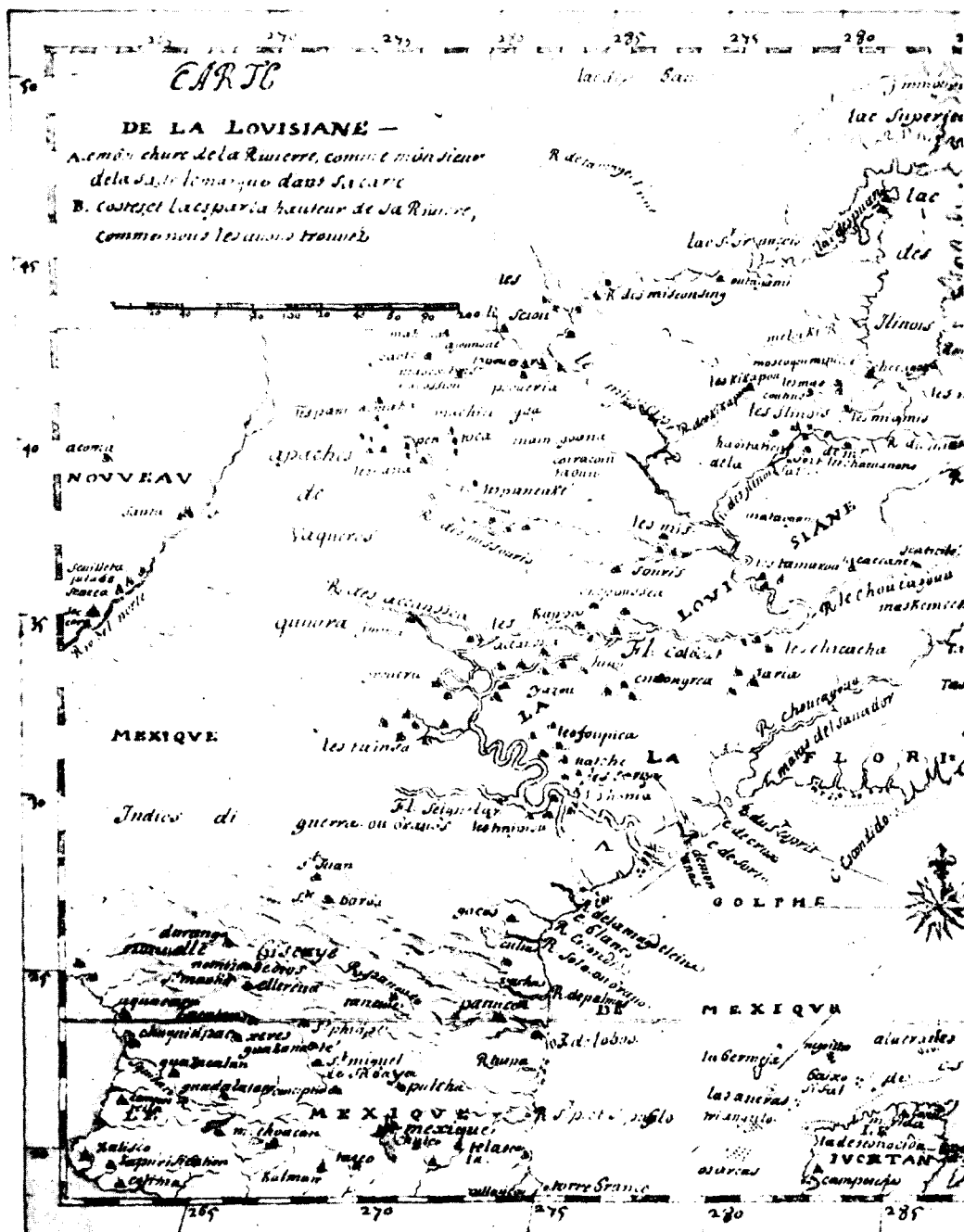
At Santo Domingo in November, La Salle informed Beaujeu that he was going to sail aboard a second vessel when the expedition entered the gulf, and he told the captain that if their ships became separated they should seek a rendezvous first at the Bay of St. Esprit and then down the coast at 28 degrees 20 minutes where La Salle expected to find one estuary of the hydra-headed river he had explored in 1682.⁶⁸ Their eventual landfall near 29 degrees put them above the entrance of their Mississippi as they projected it and perhaps above the Bay of St. Esprit. Not surprisingly, therefore, both commanders proceeded on a southwesterly course, scanning the coast for signs of the fabled bay or of the complex river system thought to lie beyond it. Through planning as much as through luck, the two men intersected again near 28 degrees, and they speculated whether some arm of the Colbert might flow into the salty bays and lakes that lay behind the low barrier

Munilla, "Política Española en le Golfo Mexicano: Expediciones motivadas por la entrada del Caballero La Salle, 1685–1707," *Anuario de Estudios Americanos*, 12 (1955): 515.

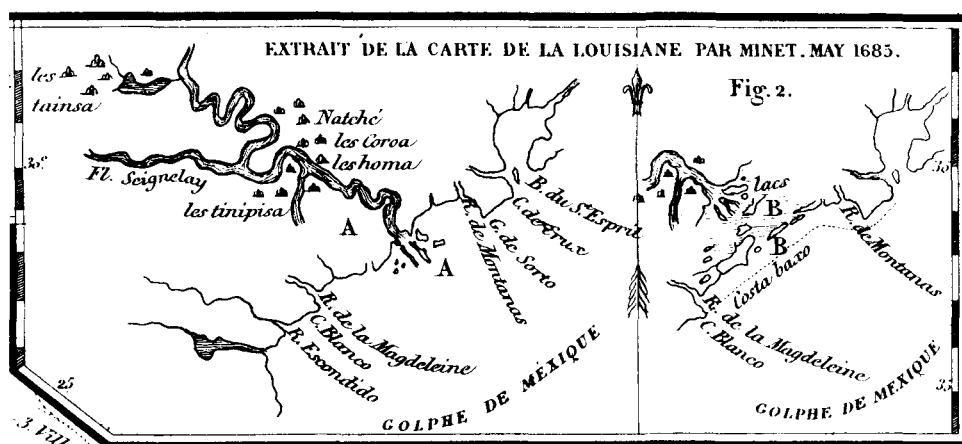
⁶⁶ John Francis Bannon, *The Spanish Borderlands Frontier, 1513–1821* (New York, 1970), 95.

⁶⁷ Weddle, *Wilderness Manhunt*, 218. The evidence confirms—modern maps of the supposed gulf route notwithstanding—that La Salle never passed or even approached the actual mouth of the Mississippi. On December 18, he steered northwest from the western tip of Cuba, taking Cape St. Anthony, with its known latitude and longitude, as his fixed point of departure. This allowed him the luxury of estimating his longitude accurately for several days as he pushed into the gulf. According to Joutel's informed account of the voyage, within forty-eight hours they were in the longitude of what is now the southwestern corner of Louisiana. And that was probably their approximate longitude when they sighted land more than a week later, for, though they continued their northwesterly course, the clockwise current of the gulf pushed them north and east. Margry, *Découvertes et Établissements des Français*, 3: 114, and 2: 505, 593. But latitude, not longitude, was the determining factor on the voyage, providing crucial north-south orientation as La Salle began to edge down the western side of the gulf in search of the Mississippi. Jean L'Archevêque, a survivor from Bayonne, told Spanish interrogators four years later that, as soon as the French had reached the coast, "they at once began to seek the latitude which Monsieur de Salle had in mind." O'Donnell, "La Salle's Occupation of Texas," 22–23.

⁶⁸ La Salle to Beaujeu, November 23, 1684, in Margry, *Découvertes et Établissements des Français*, 2: 521–23. Also see *ibid.*, 577. La Salle may have left Beaujeu's flagship because he felt uncomfortable with both commanders on one ship, especially since one of his boats had already been lost to corsairs. Or he may have simply wished to be aboard the smaller vessel, which could sail closer to shore with less fear of running aground. In response to La Salle's directions, Beaujeu told the explorer that if the ships became separated he would not miss the appointed rendezvous and would get to 28 degrees 20 minutes; *ibid.*, 524. At Cape St. Anthony on December 17, he sent La Salle a note asking him to clarify the time that they were supposed to wait for one another at the Bay of St. Esprit, their initial rendezvous in case of separation. Before setting sail the next day, La Salle replied that, if the northerly winds were obstinate, he desired Beaujeu to "wait for us at the Bay of Saint Esprit, if you go there, until the end of January, and at the Mississippi until the end of February, because in the latter place it will be easy for you to find the provisions you will need. *Ibid.*, 511, 526. Compare Minet's account in *ibid.*, 592.



Map 5: Western portion of Minet's "Map of Louisiana" (1685). Engineer Minet accompanied La Salle from France to the Gulf of Mexico and then returned to Europe with Captain Beaujeu. His chart, based on the earlier La Salle-Franquelin map, still shows the Rio del Norte flowing southwest, but it summarizes La Salle's best estimate of the Gulf Coast at the time of his colonizing expedition. The explorer believed the "Choucagoua" described by de Soto must be the Ohio, or perhaps a parallel river thought to flow into the supposed "Baye du St. Esprit." He placed the main mouth of the Mississippi (Colbert) near 28 degrees latitude, with the possibility of another mouth at 27 degrees. Minet constructed the chart for Seignelay so that a small piece of paper marked A, showing the "Mouth of the River as monsieur de la Salle marks it on his map" based on the 1682 journey, could be raised up to reveal B, "Coasts and Lakes at the latitude of his river as we found them" in the spring of 1685 (see Map 6). Reproduction from Tucker, *Indian Villages of the Illinois Country*, plate vii.



Map 6: Two versions of the western Gulf Coast, placed one over the other on Minet's chart of 1685. This detailed sketch, made in the 1850s from the original Minet map, shows how the thoughts of the French colonizers evolved as they struggled to make sense of the unexpected coastline near Matagorda Bay. When La Salle sailed from France he was assuming that the river channel he had mapped in 1682 (A) reached the gulf near 28 degrees, southwest of the traditional Bay of St. Esprit, and he suspected the passage he had seen reaching southward (Bayou La Fourche) might connect with the Magdalena or Escondido Rivers, supposed to lie near 27 degrees. Surprised to encounter the barrier islands of the Texas shoreline (B) where he had anticipated finding the Mississippi, La Salle was forced to consider the possibility that three years earlier he had actually descended into a large saltwater bay rather than into the Gulf of Mexico itself. If so, then the Mississippi entrances he remembered could still lie nearby, hidden behind the coastal islands as suggested in Figure 2. Searches made by the colonists after Minet's return to France soon revealed that hypothesis B, like hypothesis A, was incorrect. Reproduction from Thomassy, *Géologie Pratique de la Louisiane*, opposite p. 16.

islands. Given their exposed anchorage, limited provisions, and shortened tempers, La Salle, though naturally “greatly puzzled” by what he was finding, determined to disembark.⁶⁹

On January 23, in a note headed “from the mouth of one of the arms of the Colbert,” La Salle informed Beaujeu of his decision, saying, “You have brought us, happily, to the land where his Majesty has sent me, and that which remains of the way can only be made on land.” At the end of three harrying weeks, when Beaujeu prepared to sail toward the Bay of St. Esprit and then back to France, at least part of him still believed that La Salle’s river “ought, assuredly, to fall into the lake near here.”⁷⁰ But by the time he reached France in early July, he had begun to wonder whether the river was perhaps situated much further east, “in the direction of the Bay of Saint Esprit or even below it, toward the Cape of Florida.”⁷¹ The engineer and mapmaker Minet, who returned with Beaujeu, provided the most significant summary (see Map 5). “I am sending you a map of Louisiana compiled from the best maps and memorials I could find,” he wrote to Seignelay as soon as he reached port. On it, he explained, “you will see that the river [Mississippi] and the lakes of

⁶⁹ Journal of Abbé d’Esmanville, in Margry, *Découvertes et Établissements des Français*, 2: 515. At the end of the journal a letter asserts that La Salle “disembarked at 28° latitude, twenty leagues to the west of the Bay of Saint Esprit, the direct route from Cape Saint Anthony, being northwest, one quarter west”; *ibid.*, 517.

⁷⁰ Margry, *Découvertes et Établissements des Français*, 2: 526, 528; and Beaujeu to La Salle, February 18, 1685, *ibid.*, 543. In his next sentence, Beaujeu urged La Salle, “When you begin to search for it [the Mississippi], draw to the west while going out of the pond. I am the most deceived man in the world if there is not a large river there”; *ibid.* (The “lake” or “pond” was the saltwater bay where La Salle had established camp.)

⁷¹ Arnoul, the intendant at Rochefort, to Seignelay, July 8, 1685, as quoted in Delanglez, *El Rio del Espiritu Santo*, 107. What made an eastern location for the Mississippi seem plausible was not first-hand observation but second-hand information given Beaujeu when he touched at Virginia on his return voyage. The English there told him, as he informed Arnoul, “that behind the mountains that surround their country is a very large river”; *ibid.*

Canada are drawn as on his [La Salle's] map." Minet then sketched the way La Salle's river of 1682 *might* flow into the salt lagoons explored in 1685 (see Map 6) but added, "It is not certain that the river empties directly into the lakes which we saw" (that is, Aransas, San Antonio, West Matagorda, and Matagorda bays on the Texas coast). Instead, he concluded significantly, "it may just as well empty into the Bay of Saint Esprit; perhaps it is the Rio Grande."⁷² Minet's map and letter suggest the dilemma that La Salle's enterprise now faced. If some branch of his river did not flow into the salty bays where he had landed (or even if it did), where was the main trunk with its morning mists that he had traversed so smoothly in a southeasterly direction three years earlier? It might be north of these lagoons, flowing into the elusive Bay of St. Esprit—or it could be further southwest, reaching the gulf via, or near, the newly understood Rio Grande. That, in fact, was how European mapmakers such as de Rossi and Coronelli portrayed the Mississippi until the French cartographer Delisle fixed its correct location after the return of Iberville from the gulf in 1700.

The disillusioned Abbé d'Esmanville, who also returned with Beaujeu, spoke pessimistically of the explorer's situation to friends. "If what he told us is true," wrote Abbé Tronson, "M. de la Salle made a mistake, for he did not find his river at 27° or 28° latitude, as he thought he would, and only great salty lakes have been observed there that empty into the Gulf of Mexico."⁷³ But Abbé Bernou viewed reports of La Salle's position with optimism, reasoning that the explorer had so far just barely missed success and that the next steps seemed clear enough. "M. de la Salle places the mouth of his river at the 27° and a few minutes latitude," Bernou wrote in August, "[so he need not] explore those lagoons, for they are all located to the north and to the east of the place where he landed, along the northern coast of the Gulf of Mexico. All he has to do is go 20 or 30 leagues toward the *south* to find the latitude of his river, which he took with an astrolabe, and which he cannot miss, for the coast runs north-south." "This," Bernou concluded, "is evident and easy to understand."⁷⁴ Meanwhile, in Texas, La Salle must have found his situation increasingly less "evident and easy to understand." As he had intended, the explorer managed to survey part of the coast from north to south, taking advantage of the prevailing wind, and to go ashore, presumably "ten or twenty leagues north of the mouth of the river."⁷⁵ But now that he was roughly where he wanted to be,

⁷² Minet to Seignelay, July 7, 1685, in Margry, *Découvertes et Établissements des Français*, 2: 603. Using the original, Delanglez corrected Margry's date for this letter from July 6 and slightly revised the last line of Margry's version of the text. But, when Delanglez translated the passage, he inserted a nonexistent "and" (where I have put a semicolon) that again muddled an important statement. If anything, the inserted word should be "or." See Delanglez, *El Rio del Espíritu Santo*, 107.

⁷³ Abbé Tronson to Abbé Belmont, 1686, in Margry, *Découvertes et Établissements des Français*, 3: 578. Tronson added, "Perhaps his river descends into these lakes"; *ibid.* Tronson's letter was incorrectly cited by Delanglez in *Some La Salle Journeys*, 94 n. 127.

⁷⁴ Bernou to Renaudot, August 1685, as quoted in Delanglez, *Some La Salle Journeys*, 93. Compare Bernou's letter to Renaudot with Bernou to Villermont, September 4, 1685, in Margry, *Découvertes et Établissements des Français*, 2: 609–10.

⁷⁵ Memoir of Jean Cavelier, as quoted in Delanglez, *Journal of Jean Cavelier*, 136 n. 16. La Salle may have originally hoped to sail northward up the coast, before he encountered stiff winds and currents while approaching the gulf near Cape St. Anthony. "We left Santo Domingo in the latter part of November," La Salle's brother, Jean, recalled, "with the intention to reconnoiter the Mississippi and to land ten leagues north or south of the river. But the north winds having forced our three ships to put back several times, my brother at last determined to hug the wind and to steer for Florida" (that is, to sail northwestward, pointing

with a small army of several hundred persons, the dry coastal plain and the unfriendly Karankawa Indians did not fit his recollections from the earlier journey. Still, he assumed that the unusual entrance to the Mississippi must be close by.⁷⁶ In a 1689 deposition to Spanish officials, a French survivor named Jacques Grollet recalled, "Having arrived on these shores, they set out to find the place they were seeking. They should have located the place within ten days but they failed to find it in a search that lasted two months." Grollet's companion told the Spanish "that for about a year Monsieur de Salle made various attempts to find the Mississippi."⁷⁷

Far from wandering aimlessly as historians have charged, La Salle's troop seems to have made the most of its desperate circumstances, keeping alive the prospect of further discovery and conquest in the face of isolation and discouragement. They established a base camp called Fort St. Louis on the prairie beyond Matagorda Bay, and from there they began to unravel the region's baffling geography. When destruction of their only remaining boat prevented risky searches up or down the coast, they focused their attention inland. They confirmed the actual whereabouts of the Spanish and located Indians willing to join in raids on the frontier. Spanish officials in Mexico City, still baffled as to the location of the French, feared the worst. An adviser to the viceroy warned in 1686 that Spain had "to thwart the evil intention of these pirates, by which they would settle as far as New Mexico and make themselves Lords of many Kingdoms and Provinces."⁷⁸

In April 1686 La Salle went north, hoping to gain knowledge of interior groups, solve the riddle of his river, and establish contact with Tonti's outpost, all in one masterful stroke. By heading northward he expected to strike his curving Mississippi far upstream if it still lay to his west. If the river somehow lay to his east, then he thought he would hit its southwestern branch, the Seignelay, and follow it downstream to the main trunk, which he could then ascend to the Illinois, where he and Tonti had established the original Fort St. Louis in 1683.⁷⁹ But owing to the

close to the wind, and to make a landfall on the north shore of the gulf, so that the northerly winds would then be astern as they edged down the western Gulf Coast toward their presumed destination); *ibid.*, 55–57.

⁷⁶ Joutel later recalled that "M. de la Salle always looked for it [the mouth of the Mississippi] between the 27° and the 28°"; Delanglez, *Some La Salle Journeys*, 93. Just northeast of Matagorda Bay beyond the Colorado and Brazos rivers, probably near Galveston Bay, stretched the invisible line separating hunter-gatherers such as the Karankawa from richer and more settled farmer nations like the Caddo. Had La Salle disembarked on the other side of that cultural boundary, the history of his colony might have been very different. See the map in Peter Gerhard, *The North Frontier of New Spain* (Princeton, 1982), 336. Members of La Salle's company at Fort St. Louis rapidly became aware that the Cenis to the north were very different in culture and language from the people near whom they took up residence; Frank Wagner, ed., *Beranger's Discovery of Aransas Pass*, Occasional Papers of the Friends of the Corpus Christi Museum, no. 8 (Corpus Christi, 1983), 25 n. 32.

⁷⁷ O'Donnell, "La Salle's Occupation of Texas," 19, 24. The chronology and direction of La Salle's attempts to find the Mississippi have not yet been fully sorted out, but his probes were surprisingly extensive. Reports reaching New Spain mentioned strangers who wore armor and traded clothes and axes to the Tejas Indians for horses and food. Some of these newcomers, with beards and pale skin like the Spaniards, traveled as far as the lower Rio Grande several times, asking questions of the natives. They told the Spaniards "that the Spaniards were not good people, but that they themselves were," and that they planned to conquer the region for themselves. The visitors inquired about the location of the silver mines and the number of Spaniards there; they distributed knives, ribbons, beads, and shirts as tokens of friendship; "they ate with them and danced at their dances and did them no harm at all," promising to treat the Indians like brothers and to return with more goods on their way to the mines at Parral. Hackett, *Historical Documents*, 269, 271, 275. Also see Weddle, *Wilderness Manhunt*, 166–69; Dunn, *Spanish and French Rivalry*, 97; and Folmer, *Franco-Spanish Rivalry*, 161–63.

⁷⁸ "Ynform" of Sebastian de Guzman y Gordoba, as quoted in Weddle, *Wilderness Manhunt*, 53.

⁷⁹ Tonti and a relief party were in fact going back up the Mississippi in April 1686, having descended the river to the gulf in search of the colonists. See Osler, *La Salle*, 233.

hardships of the journey, the party never got beyond northern Texas, and their logical strategy failed. One man was eaten by an alligator, while others fell sick or deserted to the Indians, and in August La Salle returned to Matagorda Bay with the weary survivors. In January of 1687 he set out one final time to pursue a similar course. By mid-March he was scarcely one hundred miles from the southwestern edge of the great river system he had claimed so confidently five years earlier. Had he reached the lower branches of the Seignelay (Red River), he could have pushed downstream to the Mississippi itself and perhaps eventually have gotten word of his situation back to France via Canada. Such a contact might well have preserved the colony and clarified for posterity the curious odyssey of its founder. But this was not to be. Near the Trinity River discipline within the small party finally broke down, and dissidents murdered La Salle's nephew and several others. Apparently fearing harsh punishment, the rebels then prepared an ambush and, on March 18, caught "Monsieur de Salle off guard" and killed him "by shooting him in the head with an arquebus."⁸⁰ The remnants of the colony he left behind survived less than two years. When the Spanish finally located Fort St. Louis in the spring of 1689, its last inhabitants had dispersed or died.

Once we understand the logic that drew La Salle to the Texas coast, we can reappraise the longstanding view that his entire venture was a "debacle" and that "he failed utterly because of flaws in his character."⁸¹ Clearly the explorer, in all his complexity, was more than a wilderness martyr or a deceitful lunatic, and his portrait, as Charlevoix realized long ago, has been "more disfigured by his enemies than embellished by his friends." To decipher La Salle's geographical reasoning and to clarify the premises behind his actions does not lessen the drama of his story. It does, necessarily, dislodge many of the assumptions on which standard renditions have been based, but at the same time it makes the entire tale more plausible and coherent. And this in turn invites us to move beyond the stilted and inaccurate tragedy embraced by recent generations to a re-examination of the whole corpus of La Salle documents from a fresh vantage point more in keeping with the tenets of modern scholarship. Given the tangled nature of this historical record, disagreements over routes, motives, and personality traits may never be settled entirely. But as historians labor to discard the burdening presumptions of "Great Man" hagiography and imperialist cant, broader questions begin to assume a new importance. For our generation the rediscovery of La Salle, like that of Columbus, has less to do with the fascinating character of an individual explorer than with the complicated exchanges—economic, cultural, political, and biological—touched off in America by his voyages.

⁸⁰ O'Donnell, "La Salle's Occupation of Texas," 24.

⁸¹ McWilliams, *Iberville's Gulf Journals*, 75 n. 121.

Infant Birth Weight and Nutrition in Industrializing Montreal

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THE STATE OF NUTRITION IN THE PAST is one of the most intractable historical problems. Any appraisal of former living conditions must attempt to assess the adequacy of diets. Yet most records of the past tell us little about what people ate and whether or not the diet met their needs. Descriptions of food in history—the conventional approach to the question—fail to evaluate sufficiency. Attempts to overcome this difficulty through the measurement of diet have generally either assessed whether the total food supplies available in a community were enough for its population or analyzed food price trends and tax revenues as indices of overall consumption. Institutional menus and typical working-class diets drawn up by social reformers have also been employed as models of normal regimens.¹ At best these techniques measure the availability of food without providing much evidence about its social distribution. At worst they are hypothetical analyses that offer little insight into actual dietary norms. Certainly none measures the critical factor—the human consequences of food consumption patterns.

Happily, opportunities for the study of nutrition in history have not been exhausted. Because the birth weight of infants is closely associated with the nutritional status of their mothers during pregnancy, analysis of birth weight samples offers a new and superior means of assessing the quality of diet in the past. Such an approach provides reasonably accurate measurement of the consequences

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¹ For an introduction to the history of nutrition, see John Burnett, *Plenty and Want: A Social History of Diet in England from 1815 to the Present Day* (London, 1966); J.-J. Hémardinger, ed., *Pour une histoire de l'alimentation* (Paris, 1970); "Histoire de la consommation," *Annales: Économies, sociétés, civilisations* (1975): 402–632 (a special issue with numerous essays on the history of food and diet); Elborg Forster and Robert Forster, eds., *European Diet from Pre-Industrial to Modern Times* (New York, 1975); and Derek J. Oddy and Derek Miller, eds., *The Making of the Modern British Diet* (London, 1976).

of diet, not just of the availability of food and the possibilities for adequate nutrition.

Unfortunately, here too there are pitfalls, for influences other than maternal nutrition affect the weight of a child at birth. Indeed, three sets of factors determine neonatal weight: a mother's health, her pregnancy history, and the genetic or constitutional character of both the mother and her unborn child. Nutrition has a primary influence on maternal health and thus is extremely important to both mother and infant.² The quality of maternal diet during gestation, which directly affects fetal growth, is also closely correlated with maternal weight gain during pregnancy—the most common index now employed to assess nutritional sufficiency between conception and childbirth. Undernourished women commonly bear smaller children at full term than do those whose diets are adequate.³ The precise relationship between maternal nutrition and fetal growth, however, is still far from evident. As one investigator has observed, “neither the macro and micro requirements nor the interrelationships between them needed to support optimal growth of the human fetus are known. Furthermore, the type and extent of placental processing or synthesis of nutrients being transported from mother to fetus are inadequately understood.”⁴ In spite of these limitations, it is clear that the most important dietary contribution to prenatal nutrition is made by calories. High caloric intake is closely linked with high maternal weight gain and high birth weights.⁵ Significant deprivation, however, does not immediately lower neonatal weights, as studies of famine in Holland during World War II have shown.

Ironically, medical inquiry often benefits from the afflictions of others. The tragic Dutch hunger of 1944–45 is one such case, for it provides an excellent opportunity to study the influence of malnutrition upon human physical development. Between mid-September 1944 and early May 1945 famine scourged much of northwestern Holland, then the most densely populated region in Europe. This part of the country depended substantially on imported food supplies, access to which was disrupted after the Allied invasion of the Netherlands in September 1944. The dearth was most acute during the first four months of 1945, when bread and potatoes were virtually the only foods officially available. Estimates suggest that by April the average caloric intake of adults living in Amsterdam had fallen to one-half its prewar level and to two-thirds the level at the onset of the crisis.⁶ Because much of the food then consumed came from urban philanthropies and through foraging and black-market activity, those who through old age, poverty, and

² For a review of the literature, see Jack Metcalf, “Association of Fetal Growth with Maternal Nutrition,” in Frank Falkner and J. M. Tanner, eds., *Human Growth*, volume 1: *Principles and Prenatal Growth* (New York, 1978), 415–60. For a recent comprehensive commentary, see Mervyn Susser, “Prenatal Nutrition, Birthweight, and Psychological Development: An Overview of Experiments, Quasi-Experiments, and Natural Experiments in the Past Decade,” *American Journal of Clinical Nutrition*, 34 (1981): 784–803.

³ Joyce Vermeersch, “Maternal Nutrition and the Outcome of Pregnancy,” in B. S. Worthington *et al.*, *Nutrition in Pregnancy and Lactation* (St. Louis, 1977), chap. 2.

⁴ Metcalf, “Association of Fetal Growth with Maternal Nutrition,” 416.

⁵ Susser, “Prenatal Nutrition, Birthweight, and Psychological Development,” 791.

⁶ Mean daily consumption before the war was 2,570 calories. During the early part of the famine official rations dropped to 1,876 calories, and during the later months of the famine they fell further to 1,243 calories. Zena Stein *et al.*, *Famine and Human Development: The Dutch Hunger Winter of 1944/45* (New York, 1975), 48–49.

weakness could not supplement their meager government rations suffered even more acutely. At least ten thousand deaths have been attributed to this deprivation.⁷

Among other effects, the famine had serious consequences for pregnant women and their unborn children. The mean decline in neonatal weights in western Holland was 300 grams.⁸ These effects were confined almost entirely to the infants of women deprived during the last three months of pregnancy, when infant weight gain is normally most rapid.⁹ On average, mothers exposed to the full force of deprivation during their third trimester delivered babies who were much lighter than normal. Mothers who conceived children during the crisis but delivered them more than three months after its conclusion bore children of normal weight. But the relationship between declining food consumption and weight loss was not linear: "The sharp decrease in food was accurately and immediately mirrored in the postpartum weight of the mothers. By contrast, birth-weight and other infant dimensions were slower to reflect the fall-off in food and were clearly modulated by a maternal buffer. Thus, effects on infant size were seen only when a threshold level in the diet of the affected groups had been crossed."¹⁰ This threshold appeared to be an official food ration of 1,500 calories daily.¹¹ Similar declines in neonatal weights were observed in Leningrad during the German siege from August 1941 to January 1943. There, too, grave food shortages led to an abrupt fall in mean birth weights. At the height of the crisis three city clinics reported reductions in newborn weights ranging from 410 to 600 grams.¹² Like the Dutch experience, that in the Soviet Union offers compelling evidence of the close links between nutritional adequacy and weight at birth.

The significance of prenatal nutrition was not generally recognized until the 1940s, and its influence on infant birth weight has been investigated systematically only since that time.¹³ Although clinical interest in size at birth appeared in the mid-eighteenth century, it was directed at first toward examination of the relationship between infant size and infant mortality, particularly the higher death rates among males than females.¹⁴ Not until the second half of the nineteenth century, when weight gain gradually came to be regarded as a useful index of infant health, did the weighing and measuring of newborns slowly become accepted practice among obstetricians. Its purpose was to establish a base line against which the future growth of the child could be measured.¹⁵ At no time during the century did medical science give much thought to the question of diet during pregnancy. The chief concern of early midwifery textbooks and advice manuals was that pregnant

⁷ Stein *et al.*, *Famine and Human Development*, 53.

⁸ Susser, "Prenatal Nutrition, Birthweight, and Psychological Development," 794.

⁹ Stein *et al.*, *Famine and Human Development*, 92–96.

¹⁰ Susser, "Prenatal Nutrition, Birthweight, and Psychological Development," 794.

¹¹ Stein *et al.*, *Famine and Human Development*, 94.

¹² A. N. Antonov, "Children Born during the Siege of Leningrad in 1942," *Journal of Pediatrics*, 30 (1947): 253.

¹³ Metcalf, "Association of Fetal Growth with Maternal Nutrition," 416.

¹⁴ J. M. Tanner, *A History of the Study of Human Growth* (Cambridge, 1981), 254–61.

¹⁵ Thomas E. Cone, "De Pondere Infantum Recens Natorum: The History of Weighing the Newborn Infant," *Pediatrics*, 28 (1961): 497.

women not indulge themselves in the capricious longings for which they were notorious.¹⁶ Toward the end of the century medical authorities began to mention prenatal nutrition in passing, but the counsel offered was little more than platitudinous. "Diet during pregnancy," one American text commented in 1899, "should be plain, simple, digestible, highly nutritious, and taken at regular intervals."¹⁷ On this subject the medical profession had little more to offer.

Smoking, high alcohol consumption, and some forms of drug use affect maternal health and thus also retard fetal development. In addition, heart disease, diabetes, and many other debilitating medical conditions influence fetal growth. In modern Western societies, low incomes, low levels of education, and low social class are all closely associated with low birth weight. The primary cause of this tendency is the influence of poverty on maternal nutrition. But substandard environments, unhealthy living conditions, and lack of knowledge also contribute, as does heavy physical work performed among women most often by the working poor.¹⁸

The second group of variables that affect newborn weights are particular to an individual pregnancy. A broad range of complications that can occur during pregnancy itself—toxemia, hypertension, blood-type incompatibility, and uterine infections, to name a few—hinder growth, as can congenital malformations of the child. Parity, the number of children that a woman has carried, is significant as well. First children tend to be somewhat lighter than second, third, and fourth; fifth and subsequent children are heavier than the first but lighter than the others. In addition, the temporal spacing of children can have an effect; the second of two children born close together is likely to weigh less than if their birth dates had been further apart. A woman's past pregnancies are usually a guide to her later experience, although the relationship is not causal. Someone who has previously had complications during pregnancy or who has borne an infant of low birth weight is likely to do so again. Similarly, a woman who has had a normal childbirth history is likely to continue to do so. The length of gestation is a final major determinant of birth weight in this category. Babies born before the full term of pregnancy (forty weeks, plus or minus two) tend to be lighter than average since they have not completed the normal pattern of fetal development. The shorter the period of gestation, the lower the weight at birth. The converse is not necessarily true, however, for the rate of fetal growth slows appreciably during the final weeks of pregnancy. Thus, a child born somewhat after the full gestational period may well be within the normal range of weights.

¹⁶ William Buchan, *Advice to Mothers, on the Subject of Their Own Health, and of the Means of Promoting the Health, Strength, and Beauty of Their Offspring* (1809; reprint edn., New York, 1972), 13–23.

¹⁷ C. D. Palmer, "Hygiene and Management of Pregnancy," in Charles Jewet, ed., *The Practice of Obstetrics by American Authors* (New York, 1899), 153.

¹⁸ In addition to those articles already noted, factors associated with birth weight are discussed in Janet B. Hardy and E. D. Mellits, "Relationships of Low Birth Weight to Maternal Characteristics of Age, Parity, Education, and Body Size," in Dwayne M. Reed and Fiona J. Stanley, eds., *The Epidemiology of Prematurity* (Baltimore, 1977), 105–17; Herbert C. Miller *et al.*, "Maternal Factors in the Incidence of Low Birthweight Infants among Black and White Mothers," *Pediatric Research*, 12 (1978): 1016–19; Valerie M. Dowding, "New Assessment of the Effects of Birth Order and Socioeconomic Status on Birth Weight," *British Medical Journal*, 282 (1981): 683–86; and N. Tafari *et al.*, "Effects of Maternal Undernutrition and Heavy Physical Work during Pregnancy on Birth Weight," *British Journal of Obstetrics and Gynaecology*, 87 (1980): 222–26.

Genetic and constitutional factors form a third group of variables that influence neonatal weight.¹⁹ Minor variation in mean birth weight occurs among most racial and ethnic groups. In the modern United States, for example, black infants are slightly lighter than white babies born of mothers from similar social and economic environments. Each mother also possesses a specific genetic inheritance that she passes on to her child. The physical size of the mother (itself genetically linked) is related in turn to that of her newborn infant. Simply put, the larger the mother, the larger her child is likely to be—at least within certain limits. In one major study of the relationship between infant birth weight and maternal height for weight, 56 percent of children born to mothers in the shortest and lightest group were in the lightest quartile of babies, while only 7 percent of the infants born to women in the tallest and heaviest group were in the same quartile.²⁰ Maternal age also affects newborn weights. The children born of girls in their teens (and, according to some studies, to women in their later thirties as well) are somewhat lighter than those of mothers ages twenty to thirty-five. Multiple births also reduce neonatal weights substantially.

The sex of the fetus is another constitutional factor that influences birth weight. On average, male babies are slightly heavier at parturition than females. The earliest European studies, conducted before 1850, observed discrepancies of 200 to 350 grams. Since the mid-nineteenth century these differences have dwindled to about 150 grams.²¹ In this respect, fathers have a clear, although indirect, effect on what their children weigh at birth. Through his influence on sex determination the father's major genetic contribution to the size of his child, however, is not manifested until the postnatal phase of growth.

Birth weight is important because of its implications for infant well-being during the first months of life. Babies of normal weight are less exposed to neonatal hazards than those who are small. The Dutch hunger study reveals that the famine "produced a discernable, even a profound, impact on the infant mortality of an entire population" during the first ninety days of infant life.²² Today medical practice recognizes 2,500 grams as the lower limit of normal birth weight. Newborns weighing less experience considerably higher rates of morbidity and mortality than do heavier infants. Among children born with normal weights, health risks increase as this limit is approached. Formerly, small size at birth was attributed to prematurity, whether this was the case or not. But, within the past twenty years, two different types of small babies have been distinguished: those born before the normal end of gestation, and those born at full term who are nevertheless small for their gestational age. The size of the latter is the result of combinations of the various factors already discussed.

¹⁹ Elizabeth B. Robson, "The Genetics of Birth Weight," in Falkner and Tanner, *Human Growth*, 295; and Rosanne Boldman and Dwayne M. Reed, "Worldwide Variations in Low Birth Weight," in Reed and Stanley, *Epidemiology of Prematurity*, 39–52.

²⁰ A. M. Thomson *et al.*, "The Assessment of Fetal Growth," *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 75 (1968): 907.

²¹ Tanner, *History of the Study of Human Growth*, 261; and Thomson *et al.*, "Assessment of Fetal Growth," 909.

²² Stein *et al.*, *Famine and Human Development*, 84.

Thus, a wide range of conditions affect the weight of a newborn child. The mother's constitution, her well-being, and the course of her pregnancy all have a bearing on the development of her fetus. Although some of these factors are intrinsic to mother and child, others are linked with her social and economic position. Environmental conditions particularly interest the historian of nutrition, since diet is among the major influences affecting newborn weights. For this reason, when used with care, birth weight statistics can tell us much about social and economic circumstances in the past, particularly the adequacy of diet.

THE BIRTH WEIGHTS EXAMINED IN THIS STUDY were recorded at the University Lying-in Hospital in Montreal between 1851 and 1905.²³ Founded by several members of the McGill medical faculty and a group of female philanthropists, the hospital served two complementary purposes. It provided obstetrical training for medical students at the university, and it sheltered expectant women in need of charitable maternity care. Between 1843, when the hospital opened, and 1905 it admitted just over eighty-two hundred women. The number of births ranged from 80 to 200 annually, 2 to 3 percent of all children born in the city. Most patients were poor, although a few could afford to pay a modest fee. Children were usually born at home in nineteenth-century Canada. Since there was no medical reason for preferring childbirth in hospital, women found their way to lying-in homes only when they could not afford medical attendance or when they wished to conceal their pregnancies from family and friends. Unmarried women constituted three-fifths of all patients in the University Lying-in Hospital from 1851 to 1905.²⁴ The McGill affiliate drew most of its patients from the British Canadian community in Montreal because a French, Roman Catholic refuge for unwed mothers, *Ste. Pélagie*, was founded in the city during the 1840s. Nothing else is known about the background of the patients. But the occupations of the unmarried mothers at the university hospital probably differed little from those who attended *Ste. Pélagie* during this same period: 60 percent of them were servants, 26 percent resided at home, and the remaining 14 percent followed miscellaneous callings.²⁵

Within this general framework several changes occurred in the social composition of the patients in the hospital during these years. In particular, the ethnic and religious character of the group shifted. In the 1850s Irish immigrants (Roman Catholics in particular) constituted three-quarters of all patients. Thereafter their numbers declined steadily, a reflection of the sharp drop in immigration from Ireland to Canada from the late 1860s onward. Canadian-born patients, many of

²³ University Lying-in Hospital, Register of Patients, 1843–1905, McGill University Archives, Montreal, Canada. The institution was renamed the Montreal Maternity Hospital in 1887.

²⁴ Some of these women may have been from outside Montreal, but their numbers were probably not large. Immigration from the Quebec countryside was much higher among the French- than the English-speaking inhabitants in the province, and French Catholic unwed mothers accounted for a substantial proportion of foundlings abandoned at city creches during these years. Montreal was probably not a powerful magnet for rural Anglo-Canadians pregnant outside marriage.

²⁵ Soeur Sainte-Mechtilde, "La Fille-Mère: Ses Problèmes sociaux" (M.A. thesis, University of Montreal, 1946), 185–86.

TABLE 1
Means and Standard Deviations of Birth Weights in Sample

<i>Years</i>	<i>Number of Infants</i>	<i>Mean Birth Weights (grams)</i>	<i>Standard Deviation (grams)</i>
1851–55	418	3,616	541
1856–60	205	3,537	485
1861–65	320	3,547	532
1866–70	353	3,606	500
1871–75	442	3,572	555
1876–80	396	3,514	540
1881–85	314	3,395	513
1886–90	228	3,317	536
1891–95	219	3,291	462
1896–1900	296	3,209	553
1901–05	131	3,188	505
TOTAL/MEAN	3,327	3,470	547

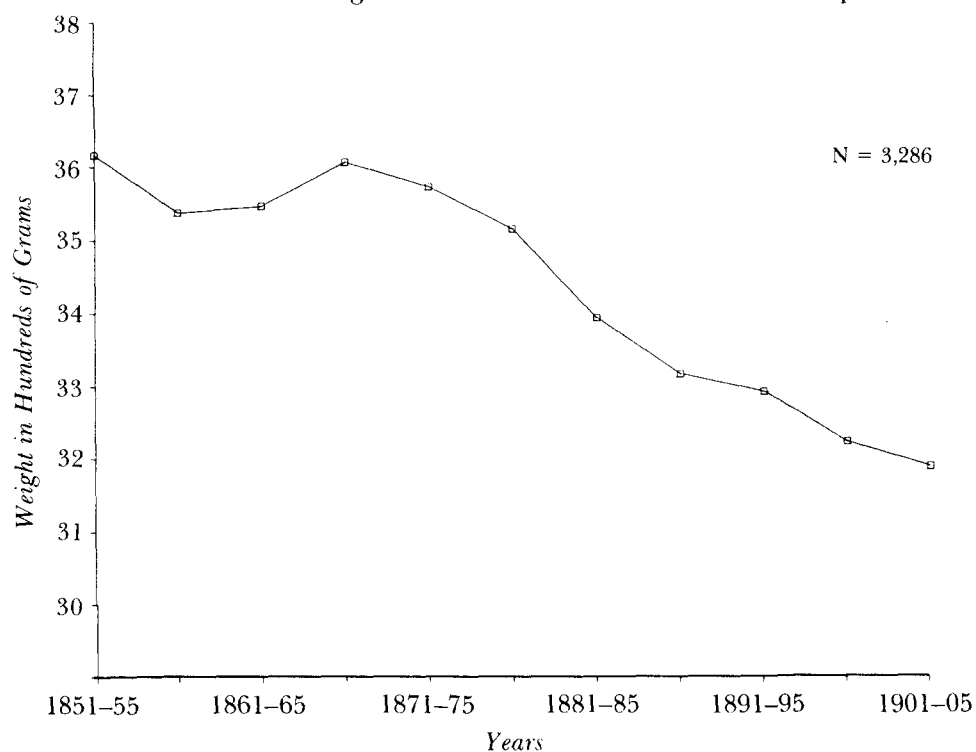
SOURCE: University Lying-in Hospital, Register of Patients, 1843–1905,
McGill University Archives, Montreal, Canada.

whom were of Irish descent, predominated after the 1860s. The number of English and Scottish immigrant patients also increased at this time. Simultaneously, the proportion of Catholic patients declined while that of Protestants grew. In the early 1850s two-thirds of all women in the hospital were Roman Catholic, most of them Irish, but by the later 1880s three-quarters of the mothers were Protestant. The number of married patients also declined. Married and unmarried women attended the hospital in more or less equal proportion during the early 1850s, but from that time onward the unmarried mothers constituted from two-thirds to three-quarters of all patients, until the turn of the century, when their numbers fell off sharply. These characteristics apart, the composition of patients in the hospital varied little over time.

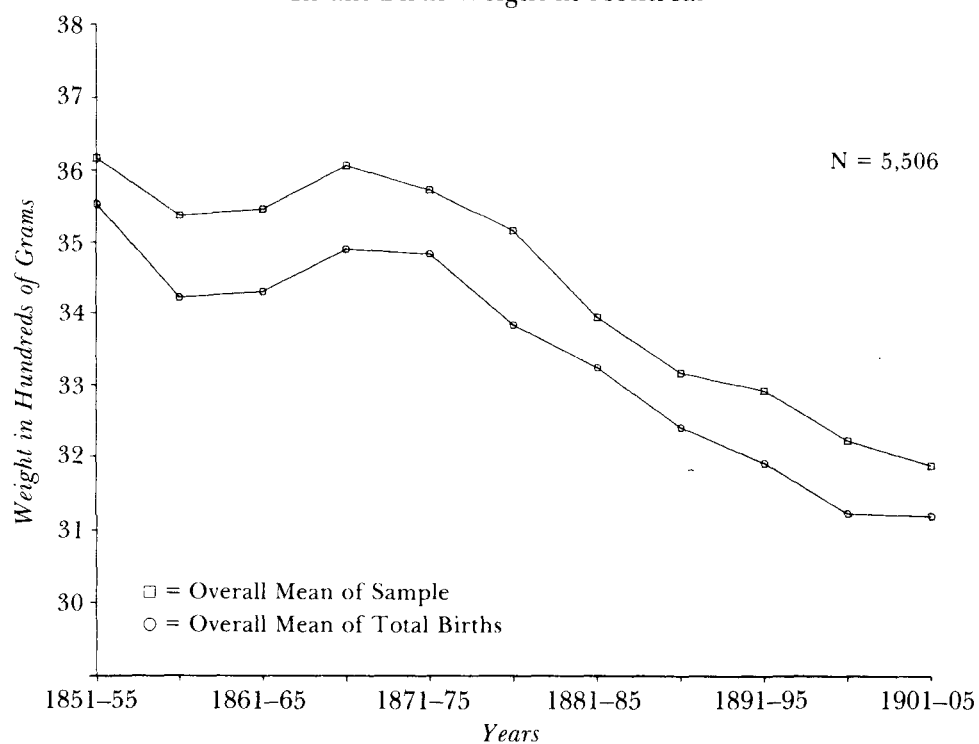
The population selected for analysis in this study consisted of all full-term, single-birth, live infants born in the hospital between 1851 and 1905, a total of 3,327 in all. The average weight of these infants was 3,470 grams.²⁶ But this average is misleading, for it masks a marked downward trend in the Montreal birth weights over time. After some fluctuation during the first twenty years of this period, the mean weights of children born in the hospital began to decline (see Graph 1) —by 100 grams in the 1870s, 200 additional grams in the 1880s, and a further 130 grams from 1890 to 1905 (see Table 1). Over all, the mean birth weights of infants delivered at the hospital fell by 430 grams (11.9 percent) during

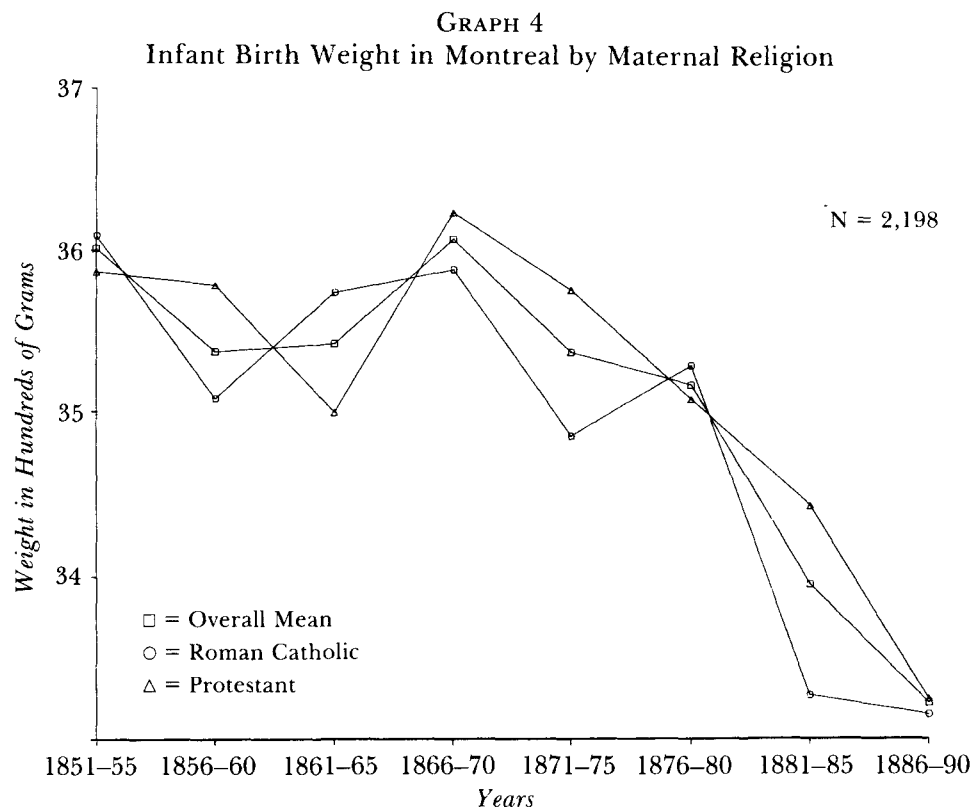
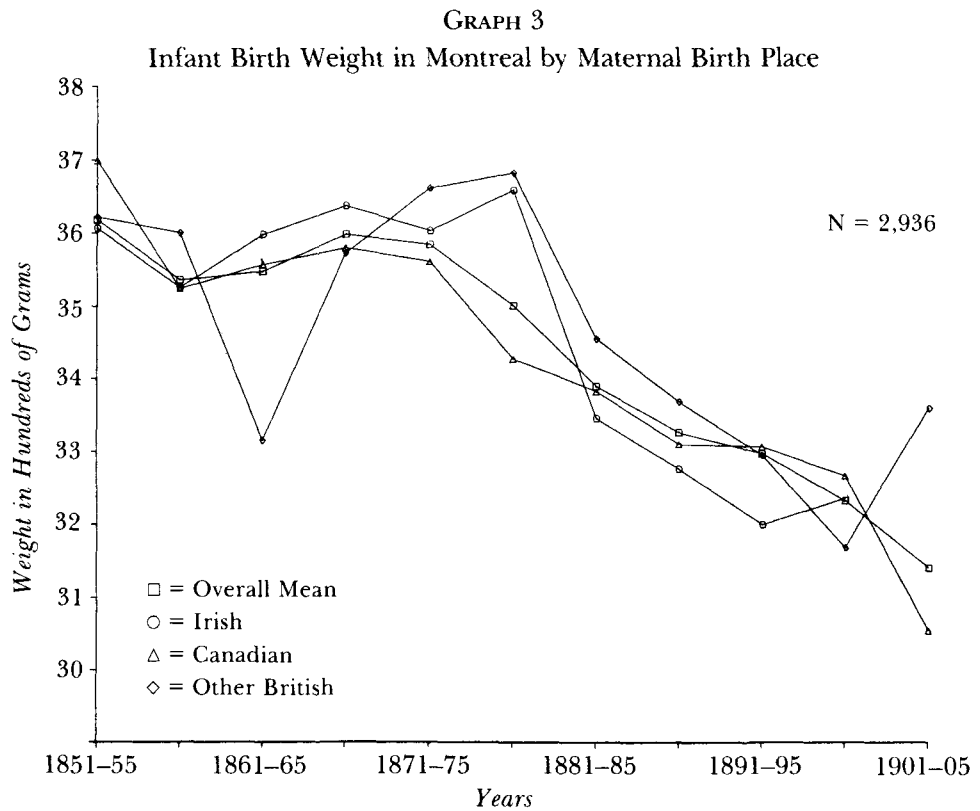
²⁶ The mean weight of all children born in the hospital for whom weights were recorded was 3,338 grams, among the highest of birth weight means reported before the end of the nineteenth century. Tanner, *History of the Study of Human Growth*, 257.

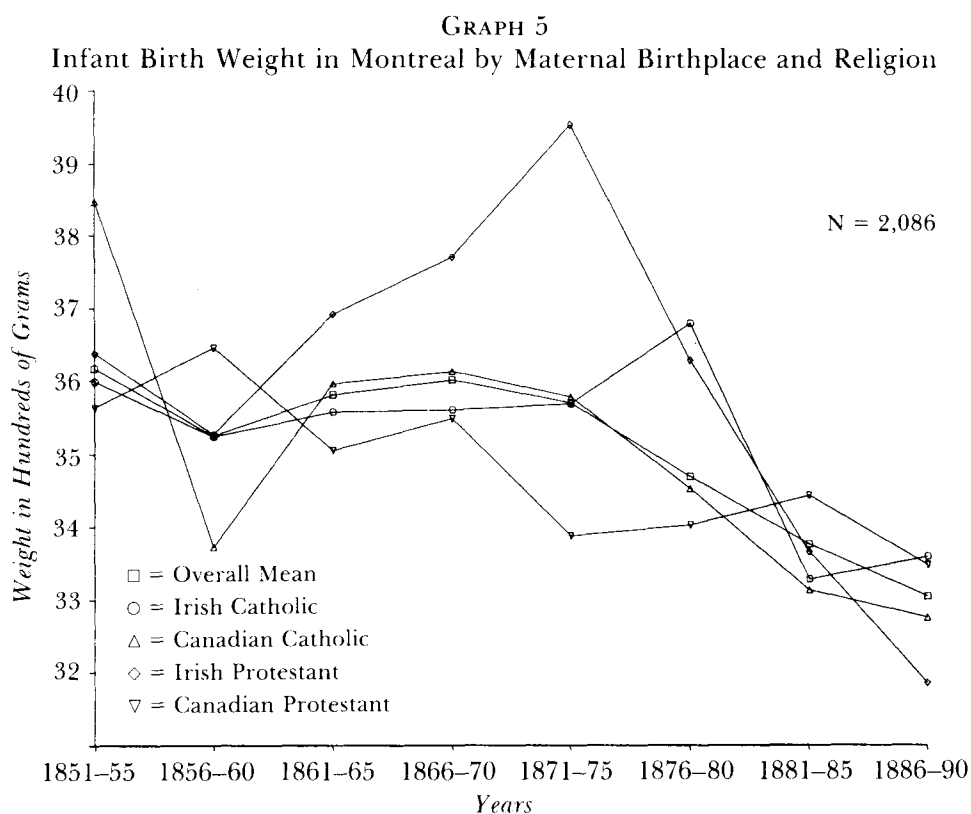
GRAPH 1
 Infant Birth Weight in Montreal: Overall Mean of Sample



GRAPH 2
 Infant Birth Weight in Montreal



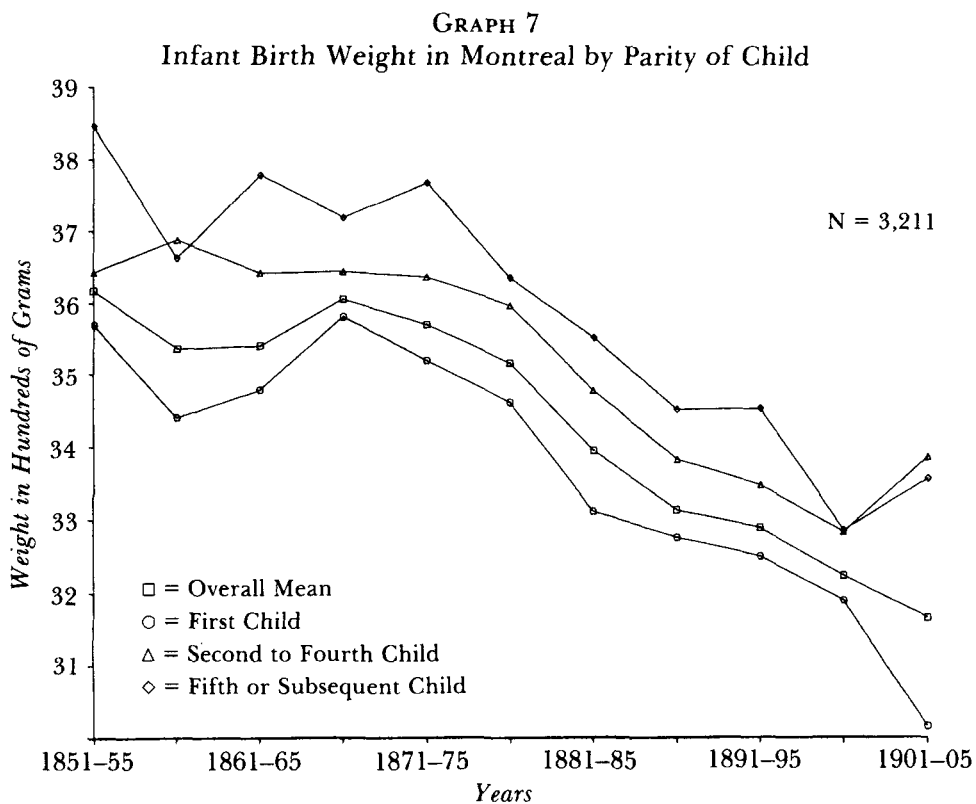
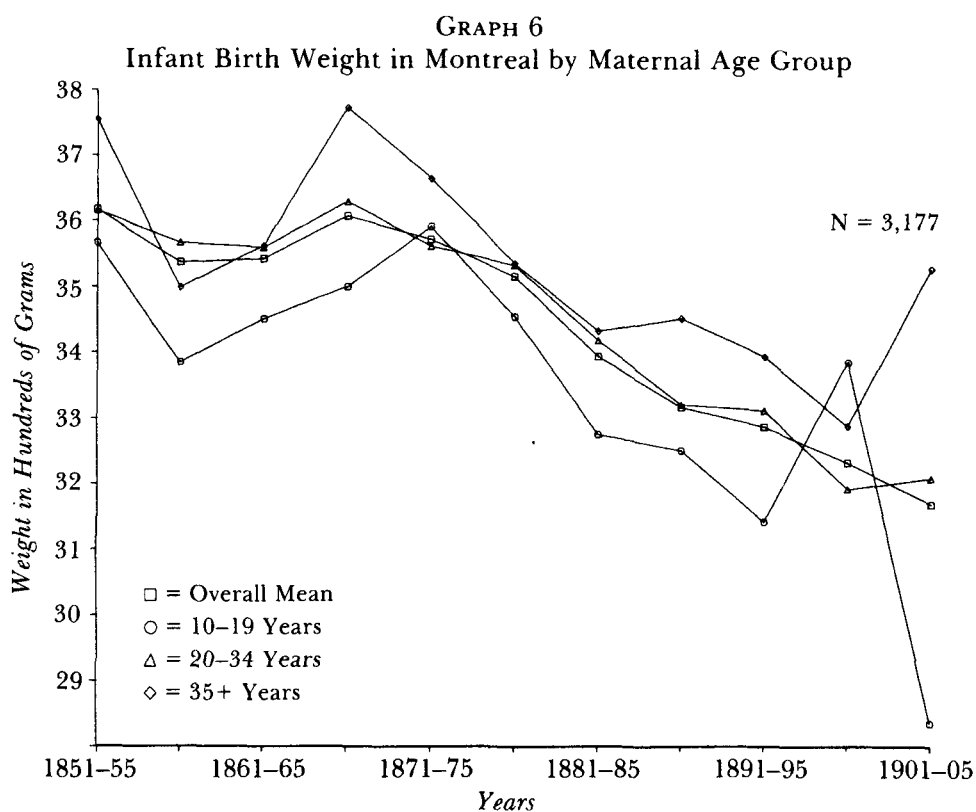


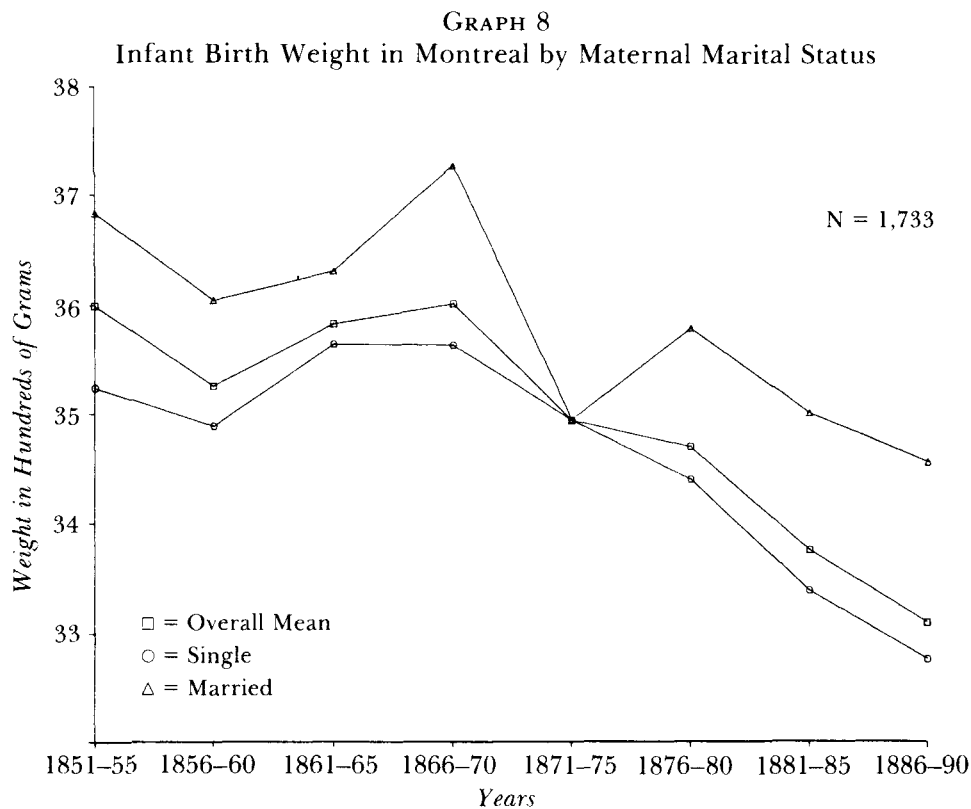


this period. This sample is, moreover, representative of all 5,506 births in the hospital for which weights were recorded (see Graph 2). The close congruence of the two curves indicates that the sample does not constitute an abnormal population.

In spite of minor variations, all national groups of patients in the hospital shared the same general experience. Women from different origins all delivered significantly smaller children in 1905 than in 1851 (see Graph 3). Three-quarters of the total were born of Irish and Canadian mothers, the former usually delivering slightly heavier infants than the latter. The weights of infants of other national groups fluctuated somewhat more erratically, a phenomenon that perhaps reflects the limited number in the sample. But the small size of these differences suggests that cultural influences played only a marginal role in affecting infant birth weight. An examination of the relationship between religion and national origin on the one hand and weight at birth on the other reveals no significant difference in the weights of Protestant and Catholic infants and only slight variations in the weights of children born to women of different national and religious groups (see Graphs 4 and 5). Irish Protestant infants were somewhat heavier than were those of Irish Catholics; the converse, however, was true of children born to Canadian mothers. These discrepancies are relatively minor when compared with the substantial decline in birth weights sustained by all groups during this period.

Graphs 6 through 8 represent the relationship of other variables to birth weights. As modern studies also indicate, younger mothers bore somewhat lighter babies on average, but, in contrast to recent findings, the oldest women in this group





produced the heaviest children of all (see Graph 6). (Here, too, the small size of the population cohort may make it somewhat less representative.) Parity also affected these weights (see Graph 7). Women bearing first children consistently had lighter infants; the overall difference was 140 grams. At the same time the group delivering fifth or subsequent children produced the heaviest babies of all; they weighed on average 240 grams more than first-born infants—again in contrast to modern findings. Thus, the interrelated variables of maternal age and parity had similar effects on birth weight trends. Unmarried women consistently bore lighter infants than did those who were married; the mean difference was just over 100 grams (see Graph 8). Far more significant than the variations caused by maternal age, parity, marital status, or place of birth and religion, however, was the pronounced general decline in the weight of newborns in all categories. These variations do not explain more than a small part of the overall decline.

WHAT CAUSED THE PROTRACTED FALL in infant birth weights? The most obvious explanation, fundamental change in the character of the sample, can be dismissed at the outset. Throughout the whole period, the hospital drew its patients almost entirely from among the poor in English-speaking Montreal. Distributional changes within the patient body, a second possibility, may have had a limited influence. The number of teenage mothers grew a little over time and may have lowered mean weights slightly, at least between 1866 and 1880 when the percentage of young mothers was highest. But during the period when weight loss was

TABLE 2
Infants Small for Their Gestational Age
and Stillbirths (Percentage)

<i>Years</i>	<i>Infants Small for Their Gestational Age</i>	<i>Stillbirths</i>
1851–55	2.9	6.1
1856–60	3.4	10.0
1861–65	2.5	6.2
1866–70	2.3	5.1
1871–75	5.0	5.4
1876–80	3.5	6.4
1881–85	6.9	4.8
1886–90	5.7	7.0
1891–95	3.2	7.4
1896–1900	9.9	9.5
1901–05	19.1	12.2

SOURCE: University Lying-in Hospital, Register of Patients, 1843–1905,
McGill University Archives, Montreal, Canada.

greatest, the proportion of young mothers fell off somewhat. If anything, this change should have offset the general decline. The sharp rise in the number of unmarried mothers may also have had adverse effects. The most abrupt increase took place during the 1850s and 1860s, however, when mean weights were relatively high; only a minor additional increase in unwed mothers occurred when birth weights plunged. Thus, changing patterns of marital status, too, contributed little to the overall decline in neonatal weights. The only other distributional changes in the sample—in religion and country of birth—had no bearing on the fall in birth weights. Thus, shifts in the character of the patient body had at most only marginal effect on the tendency toward lower birth weights.

The incidence of births of full-term, single, live-born infants weighing 2,500 grams or less confirms the general trend. The number of such children delivered in the hospital grew substantially during this period and constituted an increasing proportion of the total births over time (see Table 2). Consequently, infants who were small at full term made an important contribution to the decline in mean weights. Even when these infants are subtracted from the sample and infants of normal size are considered alone, the drop in newborn weights is striking. Between the early 1850s and the later 1890s the average weight of a normal newborn infant fell more than 330 grams. In itself, therefore, the increasing frequency of abnormally low weights at birth accounted for only one-quarter of the general decline.

Additional factors also affect birth weight. Smoking, today a primary cause of low birth weight, had little effect because few women in Canada smoked until cigarettes came into common use after World War I. Because of insufficient evidence, the

influence of other determinants cannot be dismissed so easily. Given their ready availability in pure form and as ingredients in patent medicines, alcohol and narcotics may have been a factor, although major effects on the trend would have required enormous increases in consumption. Pregnancy complications and debilitating medical conditions unrecorded by attendants at the hospital may also have increased during these years and contributed to reductions in mean birth weight. But this, too, seems unlikely. The rise in stillbirths in the hospital that occurred after 1885 is not sufficient proof of great change in pathological circumstances during pregnancy.

Maternal size, another important variable recognized in recent studies, is more difficult to determine. Unfortunately, attendants at the University Lying-in Hospital did not record maternal heights and weights. Little is known about the size distribution of Canadian women in the past. The only available source is a height table compiled for actuarial use by forty-three American and Canadian life insurance companies on the basis of policies issued between 1885 and 1909. They reveal that the average height of 136,500 insured North American women was 62.75 inches.²⁷ This corresponds almost exactly to the mean heights of American and British women reported by H. P. Bowditch, Francis Galton, and other late nineteenth-century anthropometrists.²⁸ By itself, the life insurance mean is of little use in analyzing the Montreal birth weights. Its relationship to Canadian women is unclear, and it bears an upward bias because only well-to-do women could afford to buy insurance. Presumably, poor women were somewhat shorter on average. In addition, the mean cannot convey an impression of change over time. Given the close relationship between maternal stature and infant weight, maternal size may have declined as birth weights did in response to the same underlying factors—although this cannot be verified.

Whatever the circumstances surrounding each woman's pregnancy, the complex relationship between diet and well-being as well as our present knowledge about the influence of food deprivation on fetal development both point to serious malnutrition as the primary cause of the birth-weight decline. Without full knowledge of the prenatal condition of each woman this hypothesis cannot be proved conclusively. But, given the available evidence, the long fall in Montreal birth weights cannot be convincingly explained in any other way. The quality of diet among poor women in the city seems to have deteriorated badly, particularly after 1880.

Comparing the Montreal data with that from the Dutch and Russian famine experiences during World War II is also instructive. The mean decline of 430 grams in Montreal birth weights lies mid-way between the 300-gram fall that

²⁷ The mean height was 64.25 inches, but measurements were taken while the applicants were wearing shoes. The average shoe height for women in the late Victorian and Edwardian eras has been estimated to be 1.5 inches, which has therefore been deducted. A. Hunter, "The Medico-Actuarial Investigation of Mortality of American and Canadian Life Insurance Companies," *Journal of the Institute of Actuaries*, 46 (1912): 384–94.

²⁸ They ranged between 62.5 inches and 63.33 inches. H. P. Bowditch, "The Physique of Women in Massachusetts," Commonwealth of Massachusetts, *Twenty-First Annual Report of the State Board of Health of Massachusetts* (Boston, 1890), 287–304. On the history of anthropometry, see Tanner, *History of the Study of Human Growth*; and James Allen Young, "Height, Weight, and Health: Anthropometric Study of Human Growth in Nineteenth-Century American Medicine," *Bulletin of the History of Medicine*, 53 (1979): 214–43.

occurred in western Holland in 1944–45 and the maximum 600-gram loss sustained during the much longer siege of Leningrad, both of which resulted from acute deprivation.²⁹ As the Dutch famine revealed, substantial fetal weight loss does not occur until well after the onset of maternal malnutrition. Judged by these standards, the Montreal experience was severe indeed. Both wartime hungers were caused by sudden, general food shortages. In contrast, the protracted decline in the Montreal birth weights suggests a long history of progressive malnutrition specific to one social group: poor English-speaking women. Unlike the dearth in Holland and the Soviet Union, the effects of substandard diet in Montreal were probably cumulative, and a generation of underfed mothers visited their debilities on their daughters and granddaughters when they, in turn, bore children.

WHY DID MALNUTRITION AMONG POOR WOMEN in Montreal increase during these years? Food supplies were abundant in Victorian Canada. Indeed, the cost of food declined by more than 30 percent during the last third of the nineteenth century (paralleling British and American prices), and this should have increased general standards of nutrition.³⁰ The ultimate origins of the problem seem to have been rooted in the rapidly changing economic and demographic circumstances of Montreal after 1850.

Sustained industrial development in the city began in the late 1840s, when the growth and integration of Canadian markets provided new incentives for expanded local production.³¹ The newly renovated Lachine Canal provided manufacturers with water power for heavy industrial projects, and over the next two decades grist mills, metal works, and engine foundries lined its banks. Here the Grand Trunk Railway, the largest industrial enterprise in Canada, also built machine shops. Manufacturing less dependent on hydraulic power, notably the garment and footwear trades, was located elsewhere in the city. Throughout the second half of the century the industrial base of the urban economy broadened considerably. The manufacture of tobacco products, textiles, rubber wear, transportation equipment, paper goods, chemicals, printed and published materials, and electrical appliances all increased, although clothing and boots and shoes retained their pre-eminence. By the 1890s Montreal had become the second largest industrial city in Canada. It

²⁹ Susser, "Prenatal Nutrition, Birthweight, and Psychological Development," 794; and Antonov, "Children Born during the Siege of Leningrad," 253. In both European instances the absolute drop in mean birth weights reduced averages to below the Montreal level. The Dutch decline was from 3,300 to 3,000 grams, the Russian from 3,375 to 2,800 grams. Therefore, the lowest Montreal mean, 3,190 grams, was not evidence of starvation but only of malnutrition approaching such conditions.

³⁰ Wholesale food prices fluctuated wildly between 1850 and the later 1860s. From the early 1870s a gradual downward trend is evident, its nadir reached in 1896. K. W. Taylor and H. Michell, *Statistical Contributions to Canadian Economic History*, 2 (Toronto, 1931): 47–52, 55, 87.

³¹ Gerald J. J. Tulchinsky, *The River Barons: Montreal Businessmen and the Growth of Industry and Transportation, 1837–1853* (Toronto, 1977), 203–31. Recent overviews of industrialization in Montreal and its social consequences can be found in Jean-Claude Robert, "Montréal, 1821–1871: Aspects de l'urbanisation" (Ph.D. dissertation, University of Paris, 1977), 236–304; and Paul André Linteau *et al.*, *Histoire du Québec contemporain: De la Confédération à la crise, 1867–1929* (Montreal, 1979), 138–62, 184–94. On the industrial development of Quebec, see Jean Hamelin and Yves Roby, *Histoire économique du Québec, 1851–1896* (Montreal, 1971), 261–90.

accounted for one-third of all secondary manufacturing value added in Ontario and Quebec, the industrial heartland of the country.³²

Employment in nineteenth-century Montreal was concentrated in industries producing goods for small markets and for consumers with limited buying power. The clothing, tobacco, and leather trades, in which pay rates were particularly low, dominated manufacturing. Together these industries accounted for a disproportionately small fraction of the value added per worker, one that declined substantially as the century progressed.³³ Piece work and the putting-out system, traditional devices for reducing labor costs, were used extensively in the garment trades throughout this period and, to a lesser extent, in boot and shoe production until the introduction of machines. Wages in most industries were also affected by cycles in business activity as well as by the pronounced seasonal economic fluctuations characteristic of Canadian cities. Winter was a time of shorter hours, lower wages, and higher unemployment for most Montreal workers regardless of their calling.³⁴

The major industries in Montreal relied heavily on low-cost female and child labor. In 1871 and 1881, the two census years for which this information is available, women and children constituted just over one-half of all employees in the city's five largest manufacturing activities. They predominated in garment manufacturing, the major employer in the community after 1880; they also constituted a majority in tobacco processing and a large minority in footwear production.³⁵ When nonindustrial occupations are taken into account, female and child labor made up at least 36 percent of the work force in 1871 and 41 percent a decade later. One out of three adult women and one out of four children ages ten to fourteen were wage earners in the 1870s. During the next two decades, however, these circumstances changed dramatically. According to the urban reformer Herbert Ames, the proportion of the labor force composed of women and children had declined to 23 percent by the mid-1890s.³⁶ These altered circumstances reduced female employment opportunities. At the turn of the century only a few more women were employed outside of domestic service than had been two decades earlier, even though Montreal's population had grown by 70 percent.

As industrialization reshaped the economy of Montreal, the urban population grew rapidly. A city of fifty-eight thousand in 1851, it swelled to two hundred sixty-eight thousand by the turn of the century—a mean annual growth rate of just over 3 percent. Although the precise contributions of natural increase, net migration, and the absorption of city suburbs to this gain are still unknown, the primary stimulus to population growth was migration, and this in turn had a major impact on the population structure of the city. Two of Montreal's most distinctive

³² E. J. Chambers and G. W. Bertram, "Urbanization and Manufacturing in Central Canada, 1870–1890," in Sylvia Ostry and T. K. Rymes, eds., *Canadian Political Science Association, Conference on Statistics, 1964: Papers on Regional Statistical Studies* (Toronto, 1966), 239.

³³ Chambers and Bertram, "Urbanization and Manufacturing in Central Canada," 242–53.

³⁴ Bettina Bradbury, "The Family Economy and Work in an Industrializing City: Montreal in the 1870s," in Canadian Historical Association, *Historical Papers* (1979), 76.

³⁵ *Census of Canada*, 1871, vol. 3, tables 29–53, and 1881, vol. 3, tables 29–54.

³⁶ Herbert B. Ames, *The City below the Hill* (reprint edn., Toronto, 1972), 20.

TABLE 3
Population of Montreal by Age and Sex, 1851–1901

	0–14 Years	15–29 Years	30–59 Years	60+ Years
1851				
PERCENT MALE	19.7	13.9	12.8	1.7
PERCENT FEMALE	20.0	17.9	12.3	1.5
1861				
PERCENT MALE	19.6	14.5	12.6	1.9
PERCENT FEMALE	19.0	17.6	12.7	2.1
1871*				
PERCENT MALE	19.1	13.3	12.6	1.9
PERCENT FEMALE	19.3	17.6	14.1	2.3
1881				
PERCENT MALE	16.8	13.4	13.8	2.4
PERCENT FEMALE	16.7	17.9	15.8	3.0
1891				
PERCENT MALE	15.2	14.6	14.8	2.5
PERCENT FEMALE	15.7	17.6	16.2	3.3
1901				
PERCENT MALE	14.6	14.2	15.7	2.7
PERCENT FEMALE	14.8	16.9	17.3	3.6

*The age cohorts for 1871 are 0–15, 16–30, 31–60, and 61+.

SOURCE: *Census of Canada*, 1851–1901.

demographic characteristics—its age-sex structure and its ethnic composition—were particularly affected by these migration patterns, both with important implications for the standard of living in the community.

During these years the population of Montreal gradually aged (see Table 3). The proportion of children in the city dropped substantially and that of mature and elderly people increased. These changes were, in part, a result of the progressive decline in birth and death rates that occurred everywhere in the Province of Quebec after 1850.³⁷ But the proportion of youths and young adults in the total urban population remained more or less constant throughout the second half of the century because the city was continuously infused with young migrants.³⁸ Migration also created a pronounced sex imbalance in the urban population. A consistent surplus of older girls and young women and an increasing oversupply of

³⁷ Jacques Henripin and Yves Péron, "The Demographic Transition of the Province of Quebec," in D. V. Glass and R. Revelle, eds., *Population and Social Change* (London, 1972), 213–31.

³⁸ Jean-Claude Robert, "Urbanisation et population: Le Cas de Montréal en 1861," *Revue d'histoire de l'Amérique française*, 35 (1982): 523–35.

mature women were distinctive characteristics of the urban population.³⁹ In this respect, Montreal contrasted sharply with rural Quebec, where a slight male excess in all age cohorts prevailed. Two complementary factors seem to have produced this irregularity. Female migration to the city was encouraged by the economic opportunities open to women in the city's manufacturing and service sectors; Montreal had the largest job market for women of any city in Canada. But the urban labor market must also have discouraged transient males, particularly those in search of high wages. Evidently many young men either left or bypassed the city.

The ethnic composition of Montreal also changed appreciably between 1850 and 1900. Massive British migration after the Napoleonic wars had transformed the formerly French city into one with a slight Anglo-Canadian majority. In 1851, 54 percent of the city's population was of British origin, and all but 1 percent of the remainder was French.⁴⁰ By the end of the century, however, French Canadians constituted a clear majority of city residents, while those of British background formed only 37 percent of the total. Meanwhile, substantial European immigration had established several large minorities in the city, notably Germans and East European Jews.⁴¹ In 1901 almost six times as many French lived in Montreal as had fifty years earlier; the British population increased only half as much. Most of this increase derived from in-migration and, in cultural terms, the most important source of migrants to the city was the St. Lawrence Valley, which sent thousands of rural French Canadians to the regional metropolis during its early industrialization.⁴²

The combined influence of these economic and demographic factors had a direct bearing on working-class incomes and standards of living. The first large industries in Montreal were labor-intensive enterprises that employed unskilled or semiskilled workers and, for much of this period, relied more heavily on female and child labor than did industries in other Canadian urban centers. Low rates of value added yielded low-wage employment and, when compounded by the cyclical nature of the regional economy, placed heavy pressure on working-class incomes. Sexual inequality marked the nineteenth-century wage structure, and the continued prominence of women in the local labor market reinforced this condition. The same can be said of child workers, a significant though diminishing presence in the labor force during these years.

By the second half of the nineteenth century urban residents had to rely on cash incomes to meet most of their food needs. As in other large North American cities, population density increased dramatically as Montreal grew. During the 1850s closely built housing tracts virtually eliminated garden plots and stables in some working-class districts. By the end of the century many poorer areas of the city had densities exceeding one hundred persons per acre, and a few districts had more

³⁹ D. Suzanne Cross, "The Neglected Majority: The Changing Role of Women in Nineteenth-Century Montreal," *Histoire sociale/Social History*, 6 (1973): 202–23.

⁴⁰ Fernand Ouellet, "Structures des occupations et ethnicité dans les villes de Québec et Montréal, 1819–1844," in *Éléments d'histoire sociale du Bas-Canada* (Montreal, 1972), 181; and *Census of Canada*, 1851, vol. 1, app. 3.

⁴¹ *Census of Canada*, 1901, vol. 1, table 11.

⁴² Robert, "Urbanisation et population," 531–32.

than twice that number.⁴³ Although not excessively high by European and North American urban standards, population density in Montreal was high enough to prevent wage earners from producing much meat, vegetables, and dairy products of their own. Unless food supplies could be obtained from family or friends in nearby rural districts, Montrealers had to purchase their food in local retail markets, and low incomes thus limited the quality of working-class diets. Child labor offered some amelioration, for, though modest, youthful contributions to family incomes often made an important difference to domestic food budgets. Indeed, the prevalence of child labor may be the primary reason that, in the sample, older women with several children bore heavier infants than anticipated.

A more general change in living standards was also linked to alterations in the city's ethnic structure. Mortality rates indicate that French Canadians had significantly lower standards of living than other ethnic groups in the city. Between 1876 and 1896 the death rate among the French in Montreal was twice that of the English and 50 percent higher than that of the Irish who lived in the city.⁴⁴ Although urban mortality rates declined during these years (by 26 percent among the Irish population and by just under 40 percent within the French and English communities), the proportional discrepancies were roughly as great in 1896 as they had been two decades earlier. One major component of these differential rates was infant and child mortality, which was 50 percent higher among French than English or Irish Canadians. These differences have never been adequately explained, although both nineteenth- and twentieth-century accounts emphasize overcrowding and primitive sanitary practices as primary causes. Whatever their precise origins, these variations in mortality suggest a more general pattern of differences in standards of living that set French Canadians apart from, and below, the other ethnic segments of Montreal society. The most rapidly growing cultural group in the community had the lowest standard of living, to the detriment of average living standards in the city as a whole.

In sum, the economic and demographic circumstances of Montreal after 1850 seem to have limited the possibility of general improvements in living standards for laboring people. The changing structure of the urban economy, the nature of the labor market, the composition of the work force, the high growth rate of the population, and the shifting ethnic composition of the community all undermined incomes in a city where the cash nexus had become almost the sole means of support for most of the population. The relative contributions of these influences cannot be determined. Underdevelopment, industrialization, rapid economic change, and population pressure have all been held to be decisive in low standards of living. In Montreal, these factors seem to have had cumulative effect, weighing particularly heavily on women whose economic opportunities were restricted to low-paid service and industrial jobs. At best, working women could expect only

⁴³ J. I. Cooper, "The Social Structure of Montreal in the 1850's," in Canadian Historical Association, *Report* (1956), 68. Also see Jacques Bernier, "La Condition des travailleurs, 1851-1896," in Jean Hamelin, ed., *Les Travailleurs québécois, 1851-1896* (Montreal, 1973), 47; and Ames, *City below the Hill*, 58-61.

⁴⁴ Bernier, "La Condition des travailleurs," 51; and Martin Tetreault, "L'État de santé des Montréalais, de 1880 à 1914" (M.A. thesis, University of Montreal, 1979), 55-109.

modest incomes; at worst, they might well earn a pittance for their labor. The economic situation of nonworking wives who depended on their husbands' earnings from the low-wage urban economy must often have differed little from that of women in the labor force. One clear sign of the need for additional family income was that one-third of all working-class families in the city had more than one wage earner, and most of the additional workers were children.⁴⁵ At this point, the connections that link the decline in birth weights, malnutrition, industrial development, and demographic change remain hypothetical. Without more extensive information about the social background of the hospital's patients, these connections may never be established beyond all reasonable doubt. Nevertheless, the evidence suggests that poor pregnant women suffered from growing malnutrition because industrialization and rapid population growth progressively deepened their poverty.

DOES THE ANALYSIS OF BIRTH WEIGHTS tell us anything of conditions in Montreal more generally, or is it simply an indication of distress among a small group of women at the bottom of urban society? On this question the evidence is merely suggestive. Both married and unmarried women showed signs of the same degree of nutritional deficiency over time. Given the bleak economic prospects of women pregnant outside marriage, unwed mothers might be presumed to be atypical.⁴⁶ Married patients, however, were drawn from the larger urban working class. The similarity of birth-weight histories for both groups is a sign that increasing nutritional deficiencies affected not just the poorest women but the Montreal working poor generally.

Further evidence, though not proof of growing malnutrition, confirms the low standard of living of working-class Montreal before the turn of the century. An unusually high proportion of laboring families in the city shared their homes, most often with boarders, no doubt for economic reasons.⁴⁷ Crowded housing and primitive sanitary practices were the norm in poorer districts after 1850.⁴⁸ Mortality in Montreal, while in continuous decline during these years, was consistently higher than in the province as a whole, even though urban birth rates were below the provincial average, which reduced the size of that portion of the population at greatest risk. The link between these conditions and nutrition cannot be demonstrated. Since women were particularly vulnerable to the afflictions of poverty, their

⁴⁵ Bradbury, "Family Economy and Work in an Industrializing City," 86.

⁴⁶ W. Peter Ward, "Unwed Motherhood in Nineteenth-Century English Canada," in Canadian Historical Association, *Historical Papers* (1981), 34–56.

⁴⁷ Bettina Bradbury, "The Fragmented Family: Family Strategies in the Face of Death, Illness, and Poverty—Montreal, 1860–1885," in Joy Parr, ed., *Childhood and Family in Canadian History* (Toronto, 1982), 112, and "Family Economy and Work in an Industrializing City," 90–95.

⁴⁸ George E. Fenwick, "The Medical Statistics of the City of Montreal," *British American Journal*, 2 (1861): 390–94, 439–42, 489–93, 527–31, and 3 (1862): 33–37; Philip P. Carpenter, "On the Relative Value of Human Life in Different Parts of Canada," *Canadian Naturalist*, 4 (1859): 173–86; Carpenter, "On the Vital Statistics of Montreal," *ibid.*, new ser., 3 (1866): 134–56, and "On Some of the Causes of the Excessive Mortality of Young Children in the City of Montreal," *ibid.*, 4 (1869): 188–206; and Ames, *City below the Hill*. The findings of this study also help explain the unusually high rates of infant mortality experienced in late nineteenth-century Montreal. The general decline in mean birth weights exposed more infants to the greater neonatal risks attendant upon low birth weight and thus increased the likelihood of premature death.

nutritional standards may have declined more than other groups in the urban population. Nevertheless, housing and mortality evidence is consistent with the hypothesis of generally increasing undernutrition within the Montreal working class.

Finally, the history of birth weights in Victorian Montreal raises two larger considerations. Thomas McKeown has emphasized the crucial role of improved nutrition in the modern rise of the human population. "Ten thousand years ago," he stated,

the transition from a nomadic to a settled existence, with domestication of plants and animals, resulted in an increase in food supplies, a reduction of mortality and the growth of populations; but with unrestricted expansion of numbers food supplies again became marginal. The aggregation of large, malnourished populations created the conditions required for the propagation and transmission of micro-organisms, and so led to the predominance of infectious diseases as causes of sickness and death. This established a high level of mortality which limited the rate of population growth.

The chain of influences was broken during the eighteenth and nineteenth centuries when advances in agriculture brought an increase in food supplies. The improvement in nutrition which followed led to the decline of infectious diseases and to a reduction of mortality and growth of population. This advance, unlike the earlier one associated with the first agricultural revolution, was not reversed, because declining birth rates limited the growth of population and so maintained the favourable balance between food and numbers. An additional, and in a sense quite separate, influence was a reduction of exposure to infections, particularly intestinal infections, which resulted from improvements in the quality of water and food.⁴⁹

The Montreal birth weight evidence does not question the fundamentals of this argument. Yet it does suggest that there have been important exceptions to McKeown's general conclusions. The Montreal birth weights intimate that whatever overall nutritional benefits may have accrued during these years were not evenly distributed among all social groups. Some of the poor not only failed to share in the increase but also suffered grave nutritional deficiencies. The more advantaged segments of society presumably enjoyed disproportionately large gains. By itself, the evidence gathered from one small group of maternity patients in a city on the margins of the industrial capitalist world is far from conclusive. Yet it implies that the macrocosmic approach to the study of nutrition in history obscures microcosmic conditions quite at variance with general trends. In particular, the evidence should cause us to reconsider any optimistic assumptions we might have about universal improvements in nutritional well-being during the modern era.

The birth weight data also touches directly on the debate over standards of living, a controversy now as old as the industrial age itself.⁵⁰ From Karl Marx and Frederick Engels to Eric Hobsbawm and E. P. Thompson, the pessimists have been convinced that industrialization undermined the living conditions of early industrial workers. On their part, optimists from Thomas Babington Macaulay to R. M. Hartwell have adopted an ameliorist view: living standards for most people were

⁴⁹ McKeown, *The Modern Rise of Population* (London, 1976), 162–63.

⁵⁰ For a recent introduction to the voluminous literature on this controversy, see A. J. Taylor, ed., *The Standard of Living in Britain in the Industrial Revolution* (London, 1975).

low during the early industrial era (though probably no lower than the recent preindustrial past), and economic development gradually brought about improvement. The first industrial nation, Great Britain, has been the major battle ground of the dispute, but students of industrialization in most advanced nations, including Canada, have contributed to the debate. What does birth weight evidence reveal about the broader relationship between industrialization and high population growth on one hand and nutrition and working-class living standards on the other? Was Montreal a special case, or were these circumstances shared with other modernizing communities? Without similar studies on comparable cities, these questions will go unanswered. Until answers are forthcoming we should note the signs that indicate that Montreal was particularly disadvantaged. Nineteenth-century observers repeatedly claimed that it had higher mortality rates and lower standards of housing and sanitation than other major North American communities—conditions that persisted well into the twentieth century. But these critics rarely compared like with like. Montreal began to industrialize later than most eastern American cities. The most appropriate centers with which to compare Montreal after 1850 are not the mature industrial communities of that period but communities then undergoing the great transformation. Thus far, the debate over standards of living has yielded enough evidence to indicate that malnutrition and development went hand in hand. If so, the case of the pessimists will have been dramatically affirmed.

The American Conception of National Security
and the Beginnings of the Cold War, 1945–48

MELVYN P. LEFFLER

IN AN INTERVIEW with Henry Kissinger in 1978 on “The Lessons of the Past,” Walter Laqueur observed that during World War II “few if any people thought . . . of the structure of peace that would follow the war except perhaps in the most general terms of friendship, mutual trust, and the other noble sentiments mentioned in wartime programmatic speeches about the United Nations and related topics.” Kissinger concurred, noting that no statesman, except perhaps Winston Churchill, “gave any attention to what would happen after the war.” Americans, Kissinger stressed, “were determined that we were going to base the postwar period on good faith and getting along with everybody.”¹

That two such astute and knowledgeable observers of international politics were so uninformed about American planning at the end of the Second World War is testimony to the enduring mythology of American idealism and innocence in the world of *Realpolitik*. It also reflects the state of scholarship on the interrelated areas of strategy, economy, and diplomacy. Despite the publication of several excellent overviews of the origins of the Cold War,² despite the outpouring of incisive monographs on American foreign policy in many areas of the world,³ and despite

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¹ Kissinger, *For the Record: Selected Statements, 1977–1980* (Boston, 1980), 123–24.

² For recent overviews of the origins of the Cold War, which seek to go beyond the heated traditionalist-revisionist controversies of the 1960s and early 1970s, see, for example, John L. Gaddis, *The United States and the Origins of the Cold War* (New York, 1972); Daniel Yergin, *Shattered Peace: The Origins of the Cold War and the National Security State* (Boston, 1978); Thomas G. Paterson, *On Every Front: The Making of the Cold War* (New York, 1979); and Roy Douglas, *From War to Cold War, 1942–48* (New York, 1981).

³ For some of the most important and most recent regional and bilateral studies, see, for example, Bruce Kuniholm, *The Origins of the Cold War in the Near East: Great Power Conflict and Diplomacy in Iran, Turkey, and Greece* (Princeton, 1980); Lawrence S. Wittner, *American Intervention in Greece* (New York, 1982); Aaron Miller, *Search for Security: Saudi Arabian Oil and American Foreign Policy, 1939–1949* (Chapel Hill, N.C., 1980); Michael B. Stoff, *Oil, War, and American Security: The Search for a National Policy on Foreign Oil, 1941–47* (New Haven, 1980); Timothy Ireland, *Creating the Entangling Alliance: The Origins of the North Atlantic Treaty Organization* (Westport,

some first-rate studies on the evolution of strategic thinking and the defense establishment,⁴ no comprehensive account yet exists of how American defense officials defined national security interests in the aftermath of World War II. Until recently, the absence of such a study was understandable, for scholars had limited access to records pertaining to national security, strategic thinking, and war planning. But in recent years documents relating to the early years of the Cold War have been declassified in massive numbers.⁵

Conn., 1981); William W. Stueck, Jr., *The Road to Confrontation: American Policy toward China and Korea* (Chapel Hill, N.C., 1981); Charles M. Dobbs, *The Unwanted Symbol: American Foreign Policy, the Cold War, and Korea, 1945–50* (Kent, Ohio, 1981); Dorothy Borg and Waldo Heinrichs, eds., *Uncertain Years: Chinese-American Relations, 1947–50* (New York, 1980); Robert J. McMahon, *Colonialism and Cold War: The United States and the Struggle for Indonesian Independence* (Ithaca, N.Y., 1981); Robert M. Blum, *Drawing the Line: The Origin of the American Containment Policy in East Asia* (New York, 1982); Russell D. Buhite, *Soviet-American Relations in Asia, 1945–1954* (Norman, Okla., 1981); Bruce Cumings, *The Origins of the Korean War: Liberation and the Emergence of Separate Regimes, 1945–47* (Princeton, 1982); Geir Lundestad, *America, Scandinavia, and the Cold War, 1945–49* (New York, 1980); Kenneth Ray Bain, *March to Zion: United States Foreign Policy and the Founding of Israel* (College Station, Texas, 1979); Evan M. Wilson, *Decision on Palestine: How the U.S. Came to Recognize Israel* (Stanford, 1979); Robert M. Hathaway, *Ambiguous Partnership: Britain and America, 1944–47* (New York, 1981); Terry H. Anderson, *The United States, Great Britain, and the Cold War, 1944–47* (Columbia, Mo., 1981); Eduard Mark, "American Policy toward Eastern Europe and the Origins of the Cold War, 1941–46: An Alternative Interpretation," *Journal of American History* [hereafter, *JAH*], 68 (1981–82): 313–36; Michael Schaller, "Securing the Great Crescent: Occupied Japan and the Origins of Containment in Southeast Asia," *ibid.*, 69 (1982–83): 392–414; Scott Jackson, "Prologue to the Marshall Plan," *ibid.*, 65 (1978–79): 1043–68; and Michael J. Hogan, "The Search for a 'Creative Peace': The United States, European Unity, and the Origins of the Marshall Plan," *Diplomatic History*, 6 (1982): 267–85.

⁴ For recent works on strategy, the national military establishment, and the emergence of the national security bureaucracy, see, for example, Richard Haynes, *The Awesome Power: Harry S. Truman as Commander in Chief* (Baton Rouge, La., 1973); Alfred D. Sander, "Truman and the National Security Council, 1945–1947," *JAH*, 59 (1972–73): 369–88; Michael S. Sherry, *Preparing for the Next War: American Plans for Postwar Defense, 1941–45* (New Haven, 1977); Brian L. Villa, "The U.S. Army, Unconditional Surrender, and the Potsdam Declaration," *JAH*, 63 (1976–77): 66–92; James F. Schnabel, *The History of the Joint Chiefs of Staff: The Joint Chiefs of Staff and National Policy*, volume 1: 1945–1947 (Wilmington, Del., 1979); Kenneth W. Condit, *The History of the Joint Chiefs of Staff: The Joint Chiefs of Staff and National Policy*, volume 2: 1947–49 (Wilmington, Del., 1979); Gregg Herken, *The Winning Weapon: The Atomic Bomb and the Cold War, 1945–1950* (New York, 1980); David Alan Rosenberg, "American Atomic Strategy and the Hydrogen Bomb Decision," *JAH*, 66 (1979–80): 62–87; Harry R. Borowski, *A Hollow Threat: Strategic Air Power and Containment before Korea* (Westport, Conn., 1982); Mark Stoler, "From Continentalism to Globalism: General Stanley D. Embick, the Joint Strategic Survey Committee, and the Military View of American National Policy during the Second World War," *Diplomatic History*, 6 (1982): 303–21; Walter S. Poole, "From Conciliation to Containment: The Joint Chiefs of Staff and the Coming of the Cold War," *Military Affairs*, 42 (1978): 12–16; Thomas H. Etzold, "The Far East in American Strategy, 1948–1951," in Etzold, ed., *Aspects of Sino-American Relations since 1784* (New York, 1978), 102–26; Paolo E. Coletta, *The United States Navy and Defense Unification, 1947–1953* (East Brunswick, N.J., 1981); Douglas Kinnard, *The Secretary of Defense* (Lexington, Ky., 1980); Anna K. Nelson, "National Security I: Inventing a Process, 1945–1960," in Hugh Heclo and Lester M. Salamon, eds., *The Illusion of Presidential Government* (Boulder, Col., 1981), 229–45; Larry D. O'Brien, "National Security and the New Warfare: Defense Policy, War Planning, and Nuclear Weapons, 1945–50" (Ph.D. dissertation, Ohio State University, 1981); John T. Greenwood, "The Emergence of the Post-War Strategic Air Force, 1945–1955," paper delivered at the Eighth Military History Symposium, held at the United States Air Force Academy in October 1978; and Robert F. Futrell, *Ideas, Concepts, Doctrine: A History of Basic Thinking in the United States Air Force, 1907–1964* (Maxwell Air Force Base, Ala., 1971).

⁵ For the records of the Joint Chiefs of Staff, see Record Group 218, National Archives, Washington (hereafter, RG 218); for the records of the Office of the Secretary of Defense, see Record Group 330, National Archives, Washington (hereafter, RG 330); and, for the records of the National Security Council, see Record Group 273, Judicial, Fiscal, and Social Branch, National Archives, Washington. (I used this special collection of declassified National Security Council documents prior to their receiving a record group number within the Judicial, Fiscal, and Social Branch of the National Archives.) There are important National Security Council materials in the Harry S. Truman Papers, President's Secretary's File, Harry S. Truman Presidential Library, Independence Missouri (hereafter, HTL, HSTP, PSF), boxes 191–208. For assessments by the CIA, including those prepared for meetings of the National Security Council (NSC), especially see *ibid.*, boxes 249–60, 203–07. For a helpful guide to War and Army department records in the National Archives, see Louis Galambos, ed.,

This documentation now makes it possible to analyze in greater depth the perceptions, apprehensions, and objectives of those defense officials most concerned with defining and defending the nation's security and strategic interests.⁶ This essay seeks neither to explain the process of decision making on any particular issue nor to dissect the domestic political considerations and fiscal constraints that narrowed the options available to policy makers. Furthermore, it does not pretend to discern the motivations and objectives of the Soviet Union.⁷ Rather, the goal here is to elucidate the fundamental strategic and economic considerations that shaped the definition of American national security interests in the postwar world. Several of these considerations—especially as they related to overseas bases, air transit rights, and a strategic sphere of influence in Latin America—initially were the logical result of technological developments and geostrategic experiences rather than directly related to postwar Soviet behavior.⁸ But American defense officials

The Papers of Dwight David Eisenhower, 9 vols. (Baltimore, 1970–78), 9: 2262–70. Of greatest utility in studying the views of civilian and military planners in the Army and War Department are Record Group 165, Records of the Operations Division (OPD), and Records of American-British Conversations (ABC); Record Group 319, Records of the Plans and Operations Division (P&O); Record Group 107, Records of the Office of the Secretary of War, Robert P. Patterson Papers (RPPP), safe file and general decimal file, and Records of the Office of the Assistant Secretary of War, Howard C. Peterson Papers (HCPP), classified decimal file; and Record Group 335, Records of the Under-Secretary of the Army, Draper/Voorhees files, 1947–50. The records of the navy's Strategic Plans Division (SPD) and the Politico-Military Division (PMD) are divided into many subseries; helpful indexes are available at the Naval Historical Center (NHC). The center also contains, among many other collections, the records of the Office of the Chief of Naval Operations (CNO, double zero files) as well as the manuscript collections of many influential naval officers, including Chester Nimitz, Forrest Sherman, Louis Denfeld, and Arthur Radford. For air force records, I tried—with only moderate success—to use the following materials at the National Archives: Record Group 107, Records of the Office of the Assistant Secretary of War for Air, Plans, Policies, and Agreements, 1943–47; Records of the Office of the Assistant Secretary of War for Air, Establishment of Air Fields and Air Bases, 1940–45; and Incoming and Outgoing Cablegrams, 1942–47; and Record Group 18, Records of the Office of the Chief of Air Staff, Headquarters Army Air Forces: Office of the Air Adjutant General, confidential and secret decimal correspondence file, 1945–48. For the records of the State-War-Navy Coordinating Committee and its successor, the State-Army-Navy-Air Force Coordinating Committee, see Record Group 353, National Archives, Washington, and, for the important records of the Committee of Three (meetings of the secretaries of state, war, and navy), see Record Group 107, RPPP, safe file.

⁶ I use the term “defense officials” broadly in this essay to include civilian appointees and military officers in the departments of the Army, Navy, and Air Force, in the office of the secretary of defense, in the armed services, in the intelligence agencies, and on the staff of the National Security Council. While purposefully avoiding a systematic analysis of career diplomats in the Department of State, who have received much attention elsewhere, the conclusions I draw here are based on a consideration of the views of high-ranking officials in the State Department, including James F. Byrnes, Dean Acheson, George C. Marshall, and Robert Lovett. For an excellent analysis of the views of career diplomats, see Hugh DeSantis, *The Diplomacy of Silence: The American Foreign Service, the Soviet Union, and the Cold War, 1933–1947* (Chicago, 1980). Also see, for example, Robert L. Messer, “Paths Not Taken: The United States Department of State and Alternatives to Containment, 1945–1946,” *Diplomatic History*, 1 (1977): 297–319; and W. W. Rostow, *The Division of Europe after World War II: 1946* (Austin, Texas, 1981). Many of the references in note 3 deal extensively with the views of State Department officials; see pages 346–47, above.

⁷ For recent studies of Soviet policy during this era, see, for example, Adam Ulam, *The Rivals: America and Russia since World War II* (New York, 1971), 3–151, and *Stalin: The Man and His Era* (New York, 1973), 604–99; William Zimmerman, “Choices in the Postwar World, 1: Containment and the Soviet Union,” in Charles Gati, ed., *Caging the Bear: Containment and the Cold War* (Indianapolis, 1974), 85–108; Vojtech Mastny, *Russia's Road to the Cold War* (New York, 1979); William O. McCagg, *Stalin Embattled, 1943–1948* (Detroit, 1978); William Taubman, *Stalin's American Policy: From Entente to Detente to Cold War* (New York, 1982); Werner G. Hahn, *Postwar Soviet Policies: The Fall of Zhadanov and the Defeat of Moderation* (Ithaca, N.Y., 1982); and Alvin Z. Rubinstein, *Soviet Foreign Policy since World War II: Imperial and Global* (Cambridge, Mass., 1981), 2–70.

⁸ Any assessment of postwar national security policy must also take note of the role of the atomic bomb in U.S. strategy and diplomacy. But, since nuclear weapons have received extensive attention elsewhere, I deal with this issue rather briefly; see pages 371–72, below. For excellent work on the atomic bomb, see, for

also considered the preservation of a favorable balance of power in Eurasia as fundamental to U.S. national security. This objective impelled defense analysts and intelligence officers to appraise and reappraise the intentions and capabilities of the Soviet Union. Rather modest estimates of the Soviets' ability to wage war against the United States generated the widespread assumption that the Soviets would refrain from military aggression and seek to avoid war. Nevertheless, American defense officials remained greatly preoccupied with the geopolitical balance of power in Europe and Asia, because that balance seemed endangered by communist exploitation of postwar economic dislocation and social and political unrest. Indeed, American assessments of the Soviet threat were less a consequence of expanding Soviet military capabilities and of Soviet diplomatic demands than a result of growing apprehension about the vulnerability of American strategic and economic interests in a world of unprecedented turmoil and upheaval. Viewed from this perspective, the Cold War assumed many of its most enduring characteristics during 1947–48, when American officials sought to cope with an array of challenges by implementing their own concepts of national security.

AMERICAN OFFICIALS FIRST BEGAN to think seriously about the nation's postwar security during 1943–44. Military planners devised elaborate plans for an overseas base system. Many of these plans explicitly contemplated the breakdown of the wartime coalition. But, even when strategic planners postulated good postwar relations among the Allies, their plans called for an extensive system of bases. These bases were defined as the nation's strategic frontier. Beyond this frontier the United States would be able to use force to counter any threats or frustrate any overt acts of aggression. Within the strategic frontier, American military predominance had to remain inviolate. Although plans for an overseas base system went through many revisions, they always presupposed American hegemony over the Atlantic and Pacific oceans. These plans received President Franklin D. Roosevelt's endorsement in early 1944. After his death, army and navy planners presented their views to President Harry S. Truman, and Army Chief of Staff George C. Marshall discussed them extensively with Secretary of State James C. Byrnes.⁹

example, Martin J. Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (New York, 1973); Barton J. Bernstein, "The Quest for Security: American Foreign Policy and International Control of Atomic Energy, 1942–1946," *JAH*, 60 (1973–74): 1003–44; Herken, *The Winning Weapon*; and Rosenberg, "American Atomic Strategy and the Hydrogen Bomb Decision." For older, but still very important accounts, see P. M. S. Blackett, *Fear, War, and the Bomb* (New York, 1949); Richard G. Hewlett and Oscar G. Anderson, *The New World: A History of the United States Atomic Energy Commission, 1939–1946* (University Park, Pa., 1962); Richard G. Hewlett and Francis Duncan, *Atomic Shield: A History of the United States Atomic Energy Commission, 1947–52* (University Park, Pa., 1969); Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam* (New York, 1965); and Herbert Feis, *The Atomic Bomb and the End of World War II* (Princeton, 1966).

⁹ Plans for America's overseas base system may be found in RG 218, Combined Chiefs of Staff (CCS) series 360 (12-9-42): Joint Strategic Survey Committee [hereafter, JSSC], "Air Routes across the Pacific and Air Facilities for International Police Force," March 15, 1943, JSSC 9/1; Joint Chiefs of Staff [JCS], "United States Military Requirements for Air Bases, Facilities, and Operating Rights in Foreign Territories," November 2, 1943, JCS 570/2; Joint War Plans Committee [hereafter, JWPC], "Overall Examination of the United States Requirements for Military Bases," August 25, 1943, JWPC 361/4; and JWPC, "Overall Examination of United States Requirements for Military Bases," September 13, 1945, JWPC 361/5 (revised). For Roosevelt's endorsement, see Roosevelt to the Department of State, January 7, 1944, *ibid.*, JWPC 361/5; for civilian-military

Two strategic considerations influenced the development of an overseas base system. The first was the need for defense in depth. Since attacks against the United States could only emanate from Europe and Asia, the Joint Chiefs of Staff concluded as early as November 1943 that the United States must encircle the Western Hemisphere with a defensive ring of outlying bases. In the Pacific this ring had to include the Aleutians, the Philippines, Okinawa, and the former Japanese mandates. Recognizing the magnitude of this strategic frontier, Admiral William E. Leahy, chief of staff to the president, explained to Truman that the joint chiefs were not thinking of the immediate future when, admittedly, no prospective naval power could challenge American predominance in the Pacific. Instead, they were contemplating the long term, when the United States might require wartime access to the resources of southeast Asia as well as "a firm line of communications from the West Coast to the Asiatic mainland, plus denial of this line in time of war to any potential enemy."¹⁰ In the Atlantic, strategic planners maintained that their minimum requirements included a West African zone, with primary bases in the Azores or Canary Islands. Leahy went even further, insisting on primary bases in West Africa itself—for example, at Dakar or Casablanca. The object of these defensive bases was to enable the United States to possess complete control of the Atlantic and Pacific oceans and keep hostile powers far from American territory.¹¹

Defense in depth was especially important in light of the Pearl Harbor experience, the advance of technology, and the development of the atomic bomb. According to the Joint Chiefs of Staff, "Experience in the recent war demonstrated conclusively that the defense of a nation, if it is to be effective, must begin beyond its frontiers. The advent of the atomic bomb reemphasizes this requirement. The farther away from our own vital areas we can hold our enemy through the possession of advanced bases . . . , the greater are our chances of surviving successfully an attack by atomic weapons and of destroying the enemy which employs them against us." Believing that atomic weapons would increase the incentive to aggression by enhancing the advantage of surprise, military planners never ceased to extol the utility of forward bases from which American aircraft could seek to intercept attacks against the United States.¹²

discussion of base requirements following the president's death, see OPD, "Extract of Conversation—Adm. Duncan and Gen. Lincoln," June 18, 1945, RG 165, OPD 336 (top secret); OPD, *Memorandum for the Record*, June 30, 1945, *ibid.*; General George A. Lincoln, "Memorandum concerning U.S. Post-War Pacific Bases," June 30, 1945, *ibid.*; and George C. Marshall to James F. Byrnes, July 23, 1945, *ibid.*

¹⁰ For Leahy's explanation, see JCS, "Strategic Areas and Trusteeships in the Pacific," October 10, 18, 1946, RG 218, ser. CCS 360 (12-9-42), JCS 1619/15, 19; JCS, "United States Military Requirements for Air Bases," November 2, 1943; JCS, "Overall Examination of United States Requirements for Military Bases and Base Rights," October 25, 1945, *ibid.*, JCS 570/40.

¹¹ JCS, "United States Military Requirements for Air Bases," November 2, 1943; JCS, Minutes of the 71st meeting, March 30, 1943, RG 218, ser. CCS 360 (12-9-42); Leahy, *Memorandum for the President*, November 15, 1943, *ibid.*; Nimitz, *Memorandum*, October 16, 1946, *ibid.*, JCS 1619/16; and Joint Planning Staff [hereafter, JPS], "Basis for the Formulation of a Post-War Military Policy," August 20, 1945, RG 218, ser. CCS 381 (5-13-45), JPS 633/6.

¹² JCS, "Statement of Effect of Atomic Weapons on National Security and Military Organization," March 29, 1946, RG 165, ser. ABC 471.6 Atom (8-17-45), JCS 477/10. Also see JCS, "Guidance as to the Military Implications of a United Nations Commission on Atomic Energy," January 12, 1946, *ibid.*, JCS 1567/26; and JCS, "Over-All Effect of Atomic Bomb on Warfare and Military Organization," October 30, 1945, *ibid.*, JCS 1477/1.

The second strategic consideration that influenced the plan for a comprehensive overseas base system was the need to project American power quickly and effectively against any potential adversary. In conducting an overall examination of requirements for base rights in September 1945, the Joint War Plans Committee stressed that World War II demonstrated the futility of a strategy of static defense. The United States had to be able to take "timely" offensive action against the adversary's capacity and will to wage war. New weapons demanded that advance bases be established in "areas well removed from the United States, so as to project our operations, with new weapons or otherwise, nearer the enemy." Scientists, like Vannevar Bush, argued that, "regardless of the potentialities of these new weapons [atomic energy and guided missiles], they should not influence the number, location, or extent of strategic bases now considered essential." The basic strategic concept underlying all American war plans called for an air offensive against a prospective enemy from overseas bases. Delays in the development of the B-36, the first intercontinental bomber, only accentuated the need for these bases.¹³

In October 1945 the civilian leaders of the War and Navy departments carefully reviewed the emerging strategic concepts and base requirements of the military planners. Secretary of the Navy James Forrestal and Secretary of War Robert P. Patterson discussed them with Admiral Leahy, the Joint Chiefs of Staff, and Secretary of State Byrnes. The civilian secretaries fully endorsed the concept of a far-flung system of bases in the Atlantic and Pacific oceans that would enhance the offensive capabilities of the United States.¹⁴ Having expended so much blood and effort capturing Japanese-held islands, defense officials, like Forrestal, naturally wished to devise a base system in the Pacific to facilitate the projection of American influence and power. The Philippines were the key to southeast Asia, Okinawa to the Yellow Sea, the Sea of Japan, and the industrial heartland of northeast Asia. From these bases on America's "strategic frontier," the United States could preserve its access to vital raw materials in Asia, deny these resources to a prospective enemy, help preserve peace and stability in troubled areas, safeguard critical sea lanes, and, if necessary, conduct an air offensive against the industrial infrastructure of any Asiatic power, including the Soviet Union.¹⁵

¹³ For the emphasis on "timely" action, see JWPC, "Overall Examination of Requirements for Military Bases" (revised), September 13, 1946; for the need for advance bases, see JCS, "Strategic Concept and Plan for the Employment of United States Armed Forces," September 19, 1945, RG 218, ser. CCS 381 (5-13-45), JCS 1518; for Bush's view, see JWPC, "Effect of Foreseeable New Developments and Counter-Measures on a Post-War Strategic Concept and Plan," August 22, 1945, *ibid.*, JWPC 394/1/M. Also see, for the evolution of strategic war plans, many of the materials in RG 218, ser. CCS 381 USSR (3-2-46).

¹⁴ For the discussions and conclusions of civilian officials, see Leahy to Patterson and Forrestal, October 9, 1945, RG 165, OPD 336 (top secret); Robert Lovett to Chief of Staff, October 12, 1945, *ibid.*; Patterson to the Secretary of Navy, October 17, 1945, *ibid.*; and Forrestal to Byrnes, October 4, 1945, RG 218, ser. CCS 360 (12-9-42). For Forrestal's views, also see Forrestal to James K. Vardaman, September 14, 1945, Mudd Library, Princeton University, James Forrestal Papers [hereafter ML, JFP], box 100; Forrestal to Byrnes, October 4, 1945, RG 218, ser. CCS 360 (12-9-42); and Vincent Davis, *Postwar Defense Policy and the U.S. Navy, 1943-1946* (Chapel Hill, N.C., 1962), 157-206, 259-66.

¹⁵ For the Philippines, see, for example, Strategy Section, OPD, "Post-War Base Requirements in the Philippines," April 23, 1945, RG 165, OPD 336 (top secret), and "Report on the Military Base Requirements in the Philippines," October 20, 1945, *ibid.* For Okinawa, see JCS, "Disposition of the Ryukyu Islands," September 10, 1946, RG 218, ser. CCS 360 (12-9-42), JCS 1619/9; JCS, "Review of United States Control Needed over the Japanese Islands," August 26, 1947, *ibid.*, JCS 1619/24; and Lincoln, "Memorandum concerning U.S. Post-War Pacific Bases," June 30, 1945.

Control of the Atlantic and Pacific oceans through overseas bases was considered indispensable to the nation's security regardless of what might happen to the wartime coalition. So was control over polar air routes. Admiral Leahy criticized a Joint Strategic Survey Committee report of early 1943 that omitted Iceland and Greenland as primary base requirements. When General S. D. Embick, the senior member of that committee, continued to question the desirability of a base in Iceland, lest it antagonize the Russians, he was overruled by Assistant Secretary of War John McCloy. McCloy charged that Embick had "a rather restricted concept of what is necessary for national defense." The first postwar base system approved by both the Joint Chiefs of Staff and the civilian secretaries in October 1945 included Iceland as a primary base area. The Joint War Plans Committee explained that American bases must control the air in the Arctic, prevent the establishment of enemy military facilities there, and support America's own striking forces. Once Soviet-American relations began to deteriorate, Greenland also was designated as a primary base for American heavy bombers and fighters because of its close proximity to the industrial heartland of the potential enemy. As the United States sought rights for bases along the Polar route in 1946 and 1947, moreover, American defense officials also hoped to thwart Soviet efforts to acquire similar rights at Spitzbergen and Bear Island.¹⁶

In the immediate postwar years American ambitions for an elaborate base system encountered many problems. Budgetary constraints compelled military planners to drop plans for many secondary and subsidiary bases, particularly in the South Pacific and Caribbean. These sacrifices merely increased the importance of those bases that lay closer to a potential adversary. By early 1948, the joint chiefs were willing to forego base rights in such places as Surinam, Curacao-Aruba, Cayenne, Nounea, and Vivi-Levu if "joint" or "participating" rights could be acquired or preserved in Karachi, Tripoli, Algiers, Casablanca, Dharan, and Monrovia. Budgetary constraints, then, limited the depth of the base system but not the breadth of American ambitions.¹⁷ Furthermore, the governments of Panama, Iceland, Denmark, Portugal, France, and Saudi Arabia often rejected or abolished the exclusive rights the United States wanted and sometimes limited the number of American personnel on such bases. Washington, therefore, negotiated a variety of arrangements to meet the objections of host governments. By early 1948, for example, the base in Iceland was operated by a civilian company under contract to the United

¹⁶ For Leahy's views, see JCS, Minutes of the 71st meeting, March 30, 1943. For the differences between Embick and McCloy, see Embick to John Hickerson, June 8, 1945, RG 165, OPD 336 (top secret); and Harrison A. Gerhardt, Memorandum for General Hull, June 16, 1945, *ibid.* For the utility of Iceland and Greenland as bases, see JWPC, "Attributes of United States Overseas Bases," November 2, 1945, RG 218, ser. CCS 360 (12-9-42), JWPC 361/10; NSC, "Report by the NSC on Base Rights in Greenland, Iceland, and the Azores," November 25, 1947, *ibid.*, NSC 2/1; and Albert C. Wedemeyer to Secretary of Defense, March 6, 1948, RG 330, box 19, CD 6-1-44 (decimal correspondence file). And, for the dilemma posed by prospective Soviet demands for similar base rights at Spitzbergen, see, for example, JCS, "Foreign Policy of the United States," February 10, 1946, RG 218, ser. CCS 092 US (12-21-45), JCS 1519/2; Department of State, *Foreign Relations of the United States* [hereafter, *FRUS*], 1947, 8 vols. (Washington, 1971-73), 1: 708-12, 766-70, and 3: 657-87, 1003-18; and Lundestad, *America, Scandinavia, and the Cold War*, 63-76.

¹⁷ See, for example, Report of the Director, Joint Staff, March 18, 1948, RG 218, ser. CCS 360 (12-9-42), Joint Strategic Plans Group [hereafter JSPG] 503/1. For the special emphasis on North African bases, see, for example, Forrestal to Truman, January 6, 1948, HTL, HSTP, PSF, box 156. And, for further evidence regarding plans for the development of the base system in 1947-48, see notes 70-71, below.

States Air Force; in the Azores, the base was manned by a detachment of Portuguese military personnel operating under the Portuguese flag, but an air force detachment serviced the American aircraft using the base. In Port Lyautey, the base was under the command of the French navy, but under a secret agreement an American naval team took care of American aircraft on the base. In Saudi Arabia, the Dharan air strip was cared for by 300 U.S. personnel and was capable of handling B-29s. Because these arrangements were not altogether satisfactory, in mid-1948 Secretary of Defense Forrestal and Secretary of the Army Kenneth Royall advocated using American economic and military assistance as levers to acquire more permanent and comprehensive base rights, particularly in Greenland and North Africa.¹⁸

Less well known than the American effort to establish a base system, but integral to the policymakers' conception of national security, was the attempt to secure military air transit and landing rights. Military planners wanted such rights at critical locations not only in the Western Hemisphere but also in North Africa, the Middle East, India, and southeast Asia. To this end they delineated a route from Casablanca through Algiers, Tripoli, Cairo, Dharan, Karachi, Delhi, Calcutta, Rangoon, Bangkok, and Saigon to Manila.¹⁹ In closing out the African–Middle East theater at the conclusion of the war, General H. W. Aurand, under explicit instructions from the secretary of war, made preparations for permanent rights at seven airfields in North Africa and Saudi Arabia.²⁰ According to a study by the Joint Chiefs of Staff, "Military air transit rights for the United States along the North African–Indian route were most desirable in order to provide access to and familiarity with bases from which offensive and defensive action might be conducted in the event of a major war, and to provide an alternate route to China and to United States Far Eastern bases." In other words, such rights would permit the rapid augmentation of American bases in wartime as well as the rapid movement of American air units from the eastern to the western flank of the U.S. base system. In order to maintain these airfields in a state of readiness, the United States would have to rely on private airlines, which had to be persuaded to locate their operations in areas designated essential to military air transit rights. In this way, airports "in being" outside the formal American base system would be available for military operations in times of crisis and war. Assistant Secretary McCloy informed the State

¹⁸ For the situation in Iceland, the Azores, and Port Lyautey, see Edmond T. Wooldridge to the General Board of the Navy, April 30, 1948, NHC, Records of the General Board 425 (ser. 315); for Saudi Arabia, see G. R. Cooper *et al.*, "Joint Report on Pertinent Observations during Recent Trip to the Mediterranean–Middle East Area," September 25, 1948, NHC, SPD, central files, 1948, A8; for more information on the Dharan base, also see *FRUS*, 1948, 5: 209–63; James L. Gormly, "Keeping the Door Open in Saudi Arabia: The U.S. and the Dharan Airfield, 1945–46," *Diplomatic History*, 4 (1980): 189–206; and, for aspirations to secure more permanent and comprehensive base rights, see Royall to Forrestal, July 28, 1948, RG 330, box 119, CD 27-1-21; and Forrestal to Royall, August 7, 1948, *ibid.* The State Department's concern with base rights in Iceland and Greenland was evident throughout the exploratory talks on a security pact; see *FRUS*, 1948, 3: 169–351. For North Africa, also see *ibid.*, 682–715.

¹⁹ JCS, "Over-All Examination of United States Requirements for Military Bases and Rights," September 27, 1945, RG 218, ser. CCS 360 (12-9-42), JCS 570/34; JPS, "Over-All Examination of Requirements for Transit Air Bases in Foreign Countries," January 8, 1946, *ibid.*, JPS 781; and JCS, "Over-All Examination of Requirements for Transit Air Bases and Air Base Rights in Foreign Countries," June 30, 1946, *ibid.*, JCS 570/52.

²⁰ Aurand to Patterson, February 7, 1946, Dwight David Eisenhower Presidential Library, Abilene, Kansas [hereafter, DDEL], H. S. Aurand Papers, box 28; Secretary of War to Secretary of State, March 17, 1946, *ibid.*

Department at the beginning of 1945 that a “strong United States air transport system, international in scope and readily adapted to military use, is vital to our air power and future national security.” Even earlier, the joint chiefs had agreed not to include South American air bases in their strategic plans so long as it was understood that commercial fields in that region would be developed with a view to subsequent military use.²¹

In Latin America, American requirements for effective national security went far beyond air transit rights. In a report written in January 1945 at Assistant Secretary McCloy’s behest, the War Department urged American collaboration with Latin American armed forces to insure the defense of the Panama Canal and the Western Hemisphere. Six areas within Latin America were considered of special significance either for strategic reasons or for their raw materials: the Panama Canal and approaches within one thousand miles; the Straits of Magellan; northeast Brazil; Mexico; the river Plate estuary and approaches within five hundred miles; and Mollendo, Peru-Antofagusta, and Chile. These areas were so “important,” Secretary of War Patterson explained to Secretary of State Marshall in early 1947, “that the threat of attack on any of them would force the United States to come to their defense, even though it were not certain that attack on the United States itself would follow.” The resources of these areas were essential to the United States, because “it is imperative that our war potential be enhanced. . . during any national emergency.”²²

While paying lip service to the United Nations and worrying about the impact of regional agreements in the Western Hemisphere on Soviet actions and American influence in Europe, the Joint Chiefs of Staff insisted that in practice non-American forces had to be kept out of the Western Hemisphere and the Monroe Doctrine had to be kept inviolate. “The Western Hemisphere is a distinct military entity, the integrity of which is a fundamental postulate of our security in the event of another world war.”²³ Developments in aviation, rockets, guided missiles, and atomic energy had made “the solidarity of the Hemisphere and its united support of the principles of the Monroe Doctrine” more important than before. Patterson told Marshall that effective implementation of the Monroe Doctrine now meant “that we not only refuse to tolerate foreign colonization, control, or the extension of a foreign political system to our hemisphere, but we take alarm from the appearance on the continent of foreign ideologies, commercial exploitation, cartel arrangements, or

²¹ JPS, “Over-All Examination of Requirements for Transit Air Bases . . .,” January 20, 1946, RG 218, ser. CCS 360 (10-9-42), JPS 781/1; and McCloy, Memorandum to the Department of State, January 31, 1945, RG 165, OPD 336 (top secret). Also see JPS, “Over-All Examination of Requirements for Transit Air Bases,” January 8, 1946; and, for the joint chiefs’ view on South American air fields, see JCS, Minutes of the 69th meeting, March 23, 1943, RG 218, CCS 360 (12-9-42).

²² P&O, “The Strategic Importance of Inter-American Military Cooperation” [January 20, 1947], RG 319, 092 (top secret). Also see H. A. Craig, “Summary,” January 5, 1945, RG 107, Records of the Assistant Secretary of War for Air, Establishment of Air Fields and Air Bases, box 216 (Latin America); and War Department, “Comprehensive Statement” [January 1945], *ibid.*

²³ JCS, “Foreign Policy of the United States,” February 10, 1946, RG 218, ser. CCS 092 United States (12-21-45), JCS 1592/2; and JCS to the Secretary of the Navy and Secretary of War, September 19, 1945, *ibid.*, ser. CCS 092 (9-10-45), JCS 1507/2. For JCS views on the Western Hemisphere, also see JCS to the Secretary of the Navy and Secretary of War, February 11, 1945, *ibid.*, ser. CCS 092 (1-18-45); JCS, “International Organization for the Enforcement of World Peace and Security,” April 14, 1945, *ibid.*, ser. CCS 092 (4-14-45), JCS 1311; and JCS, “Guidance as to Command and Control of the Armed Forces to be Placed at the Disposal of the Security Council of the United Nations,” May 26, 1946, *ibid.*, JCS 1670/5.

other symptoms of increased non-hemispheric influence. . . . The basic consideration has always been an overriding apprehension lest a base be established in this area by a potentially hostile foreign power." The United States, Patterson insisted, must have "a stable, secure, and friendly flank to the South, not confused by enemy penetration, political, economic, or military."²⁴

The need to predominate throughout the Western Hemisphere was not a result of deteriorating Soviet-American relations but a natural evolution of the Monroe Doctrine, accentuated by Axis aggression and new technological imperatives.²⁵ Patterson, Forrestal, and Army Chief of Staff Dwight D. Eisenhower initially were impelled less by reports of Soviet espionage, propaganda, and infiltration in Latin America than by accounts of British efforts to sell cruisers and aircraft to Chile and Ecuador; Swedish sales of anti-aircraft artillery to Argentina; and French offers to build cruisers and destroyers for both Argentina and Brazil.²⁶ To foreclose all foreign influence and to insure United States strategic hegemony, military officers and the civilian secretaries of the War and Navy departments argued for an extensive system of United States bases, expansion of commercial airline facilities throughout Latin America, negotiation of a regional defense pact, curtailment of all foreign military aid and foreign military sales, training of Latin American military officers in the United States, outfitting of Latin American armies with U.S. military equipment, and implementation of a comprehensive military assistance program.²⁷

The military assistance program, as embodied in the Inter-American Military Cooperation Act, generated the most interagency discord. Latin American experts in the State Department maintained that military assistance would stimulate regional conflicts, dissipate Latin American financial resources, and divert attention from economic and social issues. Before leaving office, Byrnes forcefully presented the State Department position to Forrestal and Patterson. Instead of dwelling on the consequences of military assistance for Latin America, Byrnes maintained that such a program would be too costly for the United States, would focus attention on a region where American interests were relatively unchallenged, and would undermine more important American initiatives elsewhere on the globe. "Greece and Turkey are our outposts," he declared.²⁸

²⁴ For Patterson's views, see P&O, "Strategic Importance of Inter-American Military Cooperation" [January 20, 1947]; and Patterson to Byrnes, December 18, 1946, RG 107, RPPP, safe file, box 3.

²⁵ This evaluation accords with the views of Chester J. Pach, Jr.; see his "The Containment of United States Military Aid to Latin America, 1944-1949," *Diplomatic History*, 6 (1982): 232-34.

²⁶ For fears of foreign influence, see, for example, [no signature] "Military Political Cooperation with the Other American Republics," June 24, 1946, RG 18, 092 (International Affairs), box 567; Patterson to the Secretary of State, July 31, 1946, RG 353, SWNCC, box 76; Eisenhower to Patterson, November 26, 1946, RG 107, HCPP, general decimal file, box 1 (top secret); S. J. Chamberlin to Eisenhower, November 26, 1946, *ibid.*; Minutes of the meeting of the Secretaries of State, War, and Navy, December 11, 1946, *ibid.*, RPPP, safe file, box 3; and Director of Intelligence to Director of P&O, February 26, 1947, RG 319, P&O, 091 France. For reports on Soviet espionage, see, for example, Military Intelligence Service [hereafter, MIS], "Soviet-Communist Penetration in Latin America," March 24, 1945, RG 165, OPD, 336 (top secret); and MIS, "Summary of a Study . . . on Soviet-Communist Penetration in Latin America," September 27, 1945, *ibid.*

²⁷ See, for example, Craig, "Summary," January 5, 1945; JPS, "Military Arrangements Deriving from the Act of Chapultepec Pertaining to Bases," January 14, 1946, RG 218, ser. CCS 092 (9-10-45), JPS 761/3; Patterson to Byrnes, December 18, 1946; and P&O, "Strategic Importance of Inter-American Military Cooperation" [January 20, 1947].

²⁸ Minutes of the meeting of the Secretaries of State, War, and the Navy, December 18, 1946, April 23, May 1, 1947, RG 107, RPPP, safe file, box 3; and M. B. Ridgway, Memorandum for the Assistant Secretary of War, February 1947, *ibid.*, HCPP, 092 (classified).

The secretary of state clearly did not think that Congress would authorize funds for Latin America as well as for Greece and Turkey. Although Truman favored military assistance to Latin America, competing demands for American resources in 1947 and 1948 forced both military planners and U.S. senators to give priority to Western Europe and the Near East. In June 1948 the Inter-American Military Cooperation Act died in the Senate. But this signified no diminution in American national security imperatives; indeed, it underscored Byrnes's statement of December 1946 that the "outposts" of the nation's security lay in the heart of Eurasia.²⁹

FROM THE CLOSING DAYS OF WORLD WAR II, American defense officials believed that they could not allow any prospective adversary to control the Eurasian land mass. This was the lesson taught by two world wars. Strategic thinkers and military analysts insisted that any power or powers attempting to dominate Eurasia must be regarded as potentially hostile to the United States.³⁰ Their acute awareness of the importance of Eurasia made Marshall, Thomas Handy, George A. Lincoln, and other officers wary of the expansion of Soviet influence there. Cognizant of the growth in Soviet strength, General John Deane, head of the United States military mission in Moscow, urged a tougher stand against Soviet demands even before World War II had ended. While acknowledging that the increase in Soviet power stemmed primarily from the defeat of Germany and Japan, postwar assessments of the Joint Chiefs of Staff emphasized the importance of deterring further Soviet aggrandizement in Eurasia.³¹ Concern over the consequences of Russian domina-

²⁹ Pach, "Military Aid to Latin America," 235–43.

³⁰ This view was most explicitly presented in an army paper examining the State Department's expostulation of U.S. foreign policy. See S. F. Giffin, "Draft of Proposed Comments for the Assistant Secretary of War on 'Foreign Policy'" [early February 1946], RG 107, HCPP 092 international affairs (classified). The extent to which this concern with Eurasia shaped American military attitudes is illustrated at greater length below. Here I should note that in March 1945 several of the nation's most prominent civilian experts (Frederick S. Dunn, Edward M. Earle, William T. R. Fox, Grayson L. Kirk, David N. Rowe, Harold Sprout, and Arnold Wolfers) prepared a study, "A Security Policy for Postwar America," in which they argued that the United States had to prevent any one power or coalition of powers from gaining control of Eurasia. America could not, they insisted, withstand attack by any power that had first subdued the whole of Europe or of Eurasia; see Frederick S. Dunn *et al.*, "A Security Policy for Postwar America," NHC, SPD, ser. 14, box 194, A1–2.

The postwar concept of Eurasia developed out of the revival of geopolitical thinking in the United States, stimulated by Axis aggression and strategic decision making. See, for example, the re-issued work of Sir Halford F. Mackinder. Mackinder, *Democratic Ideals and Reality* (1919; reprint edn., New York, 1942), and "The Round World and the Winning of Peace," *Foreign Affairs*, 21 (1943): 598–605. Mackinder's ideas were modified and widely disseminated in the United States, especially by intellectuals such as Nicholas John Spykman. Hans W. Weigert, Robert Strausz-Hupé, and Isaiah Bowman. Spykman flatly took exception to Mackinder's dictum, "Who controls eastern Europe rules the heartland; who rules the Heartland rules the World Island; and who rules the World Island rules the World." Instead, Spykman emphasized, "Who controls the rimland rules Eurasia; who rules Eurasia controls the destinies of the world." Spykman, *The Geography of Peace* (New York, 1944), 43. Also see Spykman, *America's Strategy in World Politics: The United States and the Balance of Power* (New York, 1942); Weigert, *Generals and Geographers: The Twilight of Geopolitics* (New York, 1942); Strausz-Hupé, *Geopolitics: The Struggle for Space and Power* (New York, 1942); Russell H. Fifield and G. Etzel Percy, *Geopolitics in Principle and Practice* (Boston, 1944); and Alfred C. Eckes, *The United States and the Global Struggle for Minerals* (Austin, Texas, 1979), 104–08.

³¹ For views of influential generals and army planners, see OPD, Memorandum, June 4, 1945, RG 165, OPD 336 (top secret). Also see the plethora of documents from May and June 1945, U.S. Military Academy, West Point, New York [hereafter, USMA], George A. Lincoln Papers [hereafter, GLP], War Department files. For Deane's advice, especially see Deane, "Revision of Policy with Relation to Russia," April 16, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JCS 1313, and *The Strange Alliance: The Story of Our Efforts at Wartime Co-operation with Russia* (New York, 1946), 84–86. For the JCS studies, see, for example, JPS, "Strategic Concept and Plan for the

tion of Eurasia helps explain why in July 1945 the joint chiefs decided to oppose a Soviet request for bases in the Dardanelles; why during March and April 1946 they supported a firm stand against Russia in Iran, Turkey, and Tripolitania; and why in the summer of 1946 Clark Clifford and George Elsey, two White House aides, argued that Soviet incorporation of any parts of Western Europe, the Middle East, China, or Japan into a communist orbit was incompatible with American national security.³²

Yet defense officials were not eager to sever the wartime coalition. In early 1944 Admiral Leahy noted the "phenomenal development" of Soviet power but still hoped for Soviet-American cooperation. When members of the Joint Postwar Committee met with their colleagues on the Joint Planning Staff in April 1945, Major General G. V. Strong argued against using U.S. installations in Alaska for staging expeditionary forces, lest such a move exacerbate Russo-American relations. A few months later Eisenhower, Lincoln, and other officers advised against creating a central economic authority for Western Europe that might appear to be an anti-Soviet bloc.³³ The American objective, after all, was to avoid Soviet hegemony over Eurasia. By aggravating Soviet fears, the United States might foster what it wished to avoid. American self-restraint, however, might be reciprocated by the Soviets, providing time for Western Europe to recover and for the British to reassert some influence on the Continent.³⁴ Therefore, many defense officials in 1945 hoped to avoid an open rift with the Soviet Union. But at the same time they were determined to prevent the Eurasian land mass from falling under Soviet and communist influence.

Studies by the Joint Chiefs of Staff stressed that, if Eurasia came under Soviet domination, either through military conquest or political and economic "assimilation," America's only potential adversary would fall heir to enormous natural resources, industrial potential, and manpower. By the autumn of 1945, military planners already were worrying that Soviet control over much of Eastern Europe and its raw materials would abet Russia's economic recovery, enhance its war-making capacity, and deny important foodstuffs, oil, and minerals to Western Europe. By the early months of 1946, Secretary Patterson and his subordinates in the War Department believed that Soviet control of the Ruhr-Rhineland industrial complex would constitute an extreme threat. Even more dangerous was the

Employment of United States Armed Forces," September 14, 1945, RG 218, ser. CCS 381 (5-13-45), JPS 744/3; and JCS, "United States Military Policy," September 17, 1945, *ibid.*, JCS 1496/2.

³² For the decision on the Dardanelles, see the attachments to JCS, "United States Policy concerning the Dardanelles and Kiel Canal" [July 1945], RG 218, ser. CCS 092 (7-10-45), JCS 1418/1; for the joint chiefs' position on Iran, Turkey, and Tripolitania, see JCS, "U.S. Security Interests in the Eastern Mediterranean," March 1946, *ibid.*, ser. CCS 092 USSR (3-27-45), JCS 1641 series; and Lincoln, Memorandum for the Record, April 16, 1946, RG 165, ser. ABC 336 Russia (8-22-43); and, for the Clifford memorandum, see Arthur Krock, *Memoirs: Sixty Years on the Firing Line* (New York, 1968), 477-82.

³³ Leahy, excerpt from letter, May 16, 1944, RG 59, lot 54D394 (Records of the Office of European Affairs), box 17. For Strong's opinion, see JPS, Minutes of the 199th meeting, April 25, 1945, RG 218, ser. CCS 334 (3-28-45); and, for the views of Eisenhower and Lincoln, see Lincoln, Memorandum for Hull, June 24, 1945, USMA, GLP, War Dept. files; and Leahy, Memorandum for the President [late June 1945], *ibid.*

³⁴ For the emphasis on expediting recovery in Western Europe, see, for example, McCloy, Memorandum for Matthew J. Connelly, April 26, 1945, HTL, HSTP, PSF, box 178; and, for the role of Britain, see, for example, Joint Intelligence Staff [hereafter, JIS], "British Capabilities and Intentions," December 5, 1945, RG 218, ser. CCS 000.1 Great Britain (5-10-45), JIS 161/4.

prospect of Soviet predominance over the rest of Western Europe, especially France.³⁵ Strategically, this would undermine the impact of any prospective American naval blockade and would allow Soviet military planners to achieve defense in depth. The latter possibility had enormous military significance, because American war plans relied so heavily on air power and strategic bombing, the efficacy of which might be reduced substantially if the Soviets acquired outlying bases in Western Europe and the Middle East or if they “neutralized” bases in Great Britain.³⁶

Economic considerations also made defense officials determined to retain American access to Eurasia as well as to deny Soviet predominance over it. Stimson, Patterson, McCloy, and Assistant Secretary Howard C. Peterson agreed with Forrestal that long-term American prosperity required open markets, unhindered access to raw materials, and the rehabilitation of much—if not all—of Eurasia along liberal capitalist lines. In late 1944 and 1945, Stimson protested the prospective industrial emasculation of Germany, lest it undermine American economic well being, set back recovery throughout Europe, and unleash forces of anarchy and revolution. Stimson and his subordinates in the Operations Division of the army also worried that the spread of Soviet power in northeast Asia would constrain the functioning of the free enterprise system and jeopardize American economic interests. A report prepared by the staff of the Moscow embassy and revised in mid-1946 by Ambassador (and former General) Walter Bedell Smith emphasized that “Soviet power is by nature so jealous that it has already operated to segregate from world economy almost all of the areas in which it has been established.” While Forrestal and the navy sought to contain Soviet influence in the Near East and to retain American access to Middle East oil, Patterson and the War Department focused on preventing famine in occupied areas, forestalling communist revolution, circumscribing Soviet influence, resuscitating trade, and preserving traditional American markets especially in Western Europe.³⁷ But American economic

³⁵ Joint Logistic Plans Committee [hereafter, JLPC], “Russian Capabilities,” November 15, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JLPC 35/9/RD; Military Intelligence Division [hereafter, MID], “Intelligence Estimate of the World Situation and Its Military Implications,” June 25, 1946, RG 319, P&O 350.05 (top secret); Joint Intelligence Committee [hereafter, JIC], “Intelligence Estimate Assuming that War between the Soviet Union the Non-Soviet Powers Breaks Out in 1956,” November 6, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JIC 374/1; and JIC, “Capabilities and Military Potential of Soviet and Non-Soviet Powers in 1946,” January 8, 1947, *ibid.*, JIC 374/2. For concern with the Ruhr-Rhineland industrial complex, especially see Patterson to the Secretary of State, June 10, 1946, RG 107, HCPP, 091 Germany (classified); and, for the concern with Western Europe, especially France, see, for example, JCS, “United States Assistance to Other Countries from the Standpoint of National Security,” April 29, 1947, in *FRUS, 1947*, 1: 734–50, esp. 739–42. Also see the General Board, “National Security and Navy Contributions Thereto for the Next Ten Years,” Enclosure D, June 25, 1948, NHC, General Board 425 (ser. 315).

³⁶ See, for example, JIS, “Military Capabilities of Great Britain and France,” November 13, 1945, RG 218, ser. CCS 000.1 Great Britain (5-10-45), JIS 211/1; JIS, “Areas Vital to Soviet War Effort,” February 12, 1946, *ibid.*, ser. CCS 092 (3-27-45), JIS 226/2; and JIS, “Supplemental Information Relative to Northern and Western Europe,” April 18, 1947, *ibid.*, JIS 275/1.

³⁷ Moscow embassy staff, “Russia’s International Position at the Close of the War with Germany,” enclosed in Smith to Eisenhower, July 12, 1946, DDEL, Dwight David Eisenhower Papers, file 1652, box 101. Also see, for example, Stimson to Roosevelt, September 15, 1944, ML, JFP, box 100; Stimson to Truman, May 16, 1945, HTL, HSTP, PSF, box 157; McCloy, Memorandum for Connelly, April 26, 1945, *ibid.*, box 178; MID, “Intelligence Estimate of the World Situation,” June 25, 1946; numerous memoranda, June 1945, USMA, GLP, War Dept. files; numerous documents, 1946 and 1947, RG 107, HCPP, 091 Germany (Classified); and Rearmament Subcommittee, Report to the Special Ad Hoc Committee, July 10, 1947, RG 165, ser. ABC

interests in Eurasia were not limited to Western Europe, Germany, and the Middle East. Military planners and intelligence officers in both the army and navy expressed considerable interest in the raw materials of southeast Asia, and, as already shown, one of the purposes of the bases they wanted was to maintain access to those resources and deny them to a prospective enemy.³⁸

While civilian officials and military strategists feared the loss of Eurasia, they did not expect the Soviet Union to attempt its military conquest. In the early Cold War years, there was nearly universal agreement that the Soviets, while eager to expand their influence, desired to avoid a military engagement. In October 1945, for example, the Joint Intelligence Staff predicted that the Soviet Union would seek to avoid war for five to ten years. In April 1946, while Soviet troops still remained in Iran, General Lincoln, the army's principal war planner, concurred with Byrnes's view that the Soviets did not want war. In May, when there was deep concern about a possible communist uprising in France, military intelligence doubted the Kremlin would instigate a coup, lest it ignite a full scale war. At a high-level meeting at the White House in June, Eisenhower stated that he did not think the Soviets wanted war; only Forrestal dissented. In August, when the Soviet note to Turkey on the Dardanelles provoked consternation in American policy-making circles, General Hoyt Vandenberg, director of central intelligence, informed President Truman that there were no signs of unusual Soviet troop movements or supply build-ups. In March 1947, while the Truman Doctrine was being discussed in Congress, the director of army intelligence maintained that the factors operating to discourage Soviet aggression continued to be decisive. In September 1947, the CIA concluded that the Soviets would not seek to conquer Western Europe for several reasons: they would recognize their inability to control hostile populations; they would fear triggering a war with the United States that could not be won; and they would prefer to gain hegemony by political and economic means. In October 1947, the Joint Intelligence Staff maintained that for three years at least the Soviet Union would take no action that would precipitate a military conflict.³⁹

Even the ominous developments during the first half of 1948 did not alter these assessments. Despite his alarmist cable of March 5, designed to galvanize congressional support for increased defense expenditures, General Lucius Clay, the

400.336 (3-20-47). For Forrestal's concern with Middle Eastern oil, see, for example, "Notes in Connection with Navy's 'Line' on Foreign Oil" [late 1944 or early 1945], ML, JFP, box 22; Minutes of the meeting of the Secretaries of State, War, and the Navy, April 17, 1946, RG 107, RPPP, safe file, box 3; and Walter Millis, ed., *The Forrestal Diaries* (New York, 1951), 272, 356-58.

³⁸ Strategy Section, OPD, "Post-War Base Requirements in the Philippines," April 23, 1945; JCS, "Strategic Areas and Trusteeships in the Pacific," October 18, 1946; MID, "Positive U.S. Action Required to Restore Normal Conditions in Southeast Asia," July 3, 1947, RG 319, P&O, 092 (top secret); and Lauris Norstad to the Director of Intelligence, July 10, 1947, *ibid.*

³⁹ JIS, "Russian Military Capabilities," October 25, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JIS 80/10; Lincoln to M. B. Gardner and F. F. Everest, April 10, 1946, RG 165, ser. ABC 336 Russia (8-22-43); O. S. P., Memorandum for Hull, May 3, 1946, *ibid.*, ser. ABC 381 (9-1-45); S. W. D., Memorandum for the Record, June 12, 1946, RG 319, P&O, 092 (top secret); Vandenberg, Memorandum for the President, August 24, 1946, HTL, HSTP, PSF, box 249; Chamberlin, "Reevaluation of Soviet Intentions," March 27, 1947, RG 165, Records of the Chief of Staff, 091 Russia (top secret); CIA, "Review of the World Situation as It Relates to the Security of the United States," September 26, 1947, HTL, HSTP, PSF, box 203; and JIC, "Soviet Military Objectives and Capabilities, 1947-50," October 27, 1947, RG 165, ser. ABC 381 USSR (3-2-46), JIC 391/1.

American military governor in Germany, did not believe war imminent. A few days later, the CIA concluded that the communist takeover in Czechoslovakia would not increase Soviet capabilities significantly and reflected no alteration in Soviet tactics. On March 16, the CIA reported to the president, "The weight of logic, as well as evidence, also leads to the conclusion that the Soviets will not resort to military force within the next sixty days." While this assessment was far from reassuring, army and navy intelligence experts concurred that the Soviets still wanted to avoid war; the question was whether war would erupt as a result of "miscalculation" by either the United States or Russia. After talking to Foreign Minister V. M. Molotov in June, Ambassador Smith concluded that Soviet leaders would not resort to active hostilities. During the Berlin blockade, army intelligence reported few signs of Soviet preparations for war; naval intelligence maintained that the Soviets desired to avoid war yet consolidate their position in East Germany. In October 1948, the Military Intelligence Division of the army endorsed a British appraisal that "all the evidence available indicates that the Soviet Union is not preparing to go to war in the near future." In December Acting Secretary of State Robert Lovett summed up the longstanding American perspective when he emphasized that he saw "no evidence that Soviet intentions run toward launching a sudden military attack on the western nations at this time. It would not be in character with the tradition or mentality of the Soviet leaders to resort to such a measure unless they felt themselves either politically extremely weak, or militarily extremely strong."⁴⁰

Although American defense officials recognized that the Soviets had substantial military assets, they remained confident that the Soviet Union did not feel extremely strong. Military analysts studying Russian capabilities noted that the Soviets were rapidly mechanizing infantry units and enhancing their firepower and mobility. It was estimated during the winter of 1946–47 that the Soviets could mobilize six million troops in thirty days and twelve million in six months, providing sufficient manpower to overrun all important parts of Eurasia. The Soviets were also believed to be utilizing German scientists and German technological know-how to improve their submarine force, develop rockets and missiles, and acquire knowledge about the atomic bomb. During 1947 and 1948, it was reported as well that the Soviets were making rapid progress in the development of high performance jet fighters and already possessed several hundred intermediate range bombers comparable to the American B-29.⁴¹

⁴⁰ CIA, "Special Evaluation No. 27," March 16, 1948, RG 319, P&O, 350.05 (top secret); MID, "Intelligence Division Daily Briefing," October 18, 1948, *ibid.*; and Lovett to John L. Sullivan, December 20, 1948, NHC, double zero files, 1948, box 2. Also see Jean Edward Smith, *The Papers of General Lucius D. Clay: Germany, 1945–1949*, 2 vols. (Bloomington, Ind., 1974), 2: 564–65, 568–69, 602; CIA, "Review of the World Situation," March 10, 1948, HTL, HSTP, PSF, box 203; R. H. Hillenkoetter, Memorandum for the President, March 16, 1948, *ibid.*, box 249; Chamberlin, Memorandum to the Chief of Staff, March 14, 1948, RG 319, P&O, 092 (top secret); Thomas B. Inglis, Memorandum of Information, March 16, September 28, 1948, NHC, SPD, central files, 1948, A8 and EF30; Smith to Kennan, June 11, 1948, ML, George F. Kennan Papers [hereafter, GFKP], box 28; and Carter Clarke to the Chief of Staff, August 6, 1948, RG 330, CD 2-2-2, box 4.

⁴¹ For reports on Soviet mobilization capabilities and conventional strength on the land, see, for example, Chamberlin, Memorandum to the Chief of Staff, March 14, 1948; Carter Clarke, "Present Capability of the U.S.S.R. Armed Forces," September 16, 1946, RG 319, P&O, 091 Russia (top secret); and P&O, "Capabilities (Ground) and Intentions of the USSR for Overrunning Northern and Western Europe in 1947, 1948, and 1949," February 28, 1947, *ibid.*, 350.05 (top secret). The war plans of the Joint Chiefs of Staff outline the extensive ground capabilities of Soviet forces. See especially the documents in RG 218, ser. CCS 381 USSR

Even so, American military analysts were most impressed with Soviet weaknesses and vulnerabilities. The Soviets had no long-range strategic air force, no atomic bomb, and meager air defenses. Moreover, the Soviet navy was considered ineffective except for its submarine forces.⁴² The Joint Logistic Plans Committee and the Military Intelligence Division of the War Department estimated that the Soviet Union would require approximately fifteen years to overcome wartime losses in manpower and industry, ten years to redress the shortage of technicians, five to ten years to develop a strategic air force, fifteen to twenty-five years to construct a modern navy, ten years to refurbish military transport, ten years (or less) to quell resistance in occupied areas, fifteen to twenty years to establish a military infrastructure in the Far East, three to ten years to acquire the atomic bomb, and an unspecified number of years to remove the vulnerability of the Soviet rail-net and petroleum industry to long-range bombing.⁴³ For several years at least, the Soviet capability for sustained attack against North America would be very limited. In January 1946 the Joint Intelligence Staff concluded that "the offensive capabilities of the United States are manifestly superior to those of the U.S.S.R. and any war between the U.S. and the USSR would be far more costly to the Soviet Union than to the United States."⁴⁴

Key American officials like Lovett, Clifford, Eisenhower, Bedell Smith and Budget Director James Webb were cognizant of prevailing Soviet weaknesses and potential American strength. Despite Soviet superiority in manpower, General Eisenhower and Admiral Forrest E. Sherman doubted that Russia could mount a surprise attack, and General Lincoln, Admiral Cato Glover, and Secretaries Patterson and Forrestal believed that Soviet forces would encounter acute logistical problems in trying to overrun Eurasia—especially in the Near East, Spain, and Italy. Even Forrestal doubted reports of accelerating Soviet air capabilities. American experts believed that most Soviet planes were obsolescent, that the Soviets had insufficient airfields and aviation gas to use their new planes, and that these planes had serious problems in their instrumentation and construction.⁴⁵

(3-2-46); for information on Soviet use of German scientists, see, for example, JIS, "Capabilities and Intentions of the USSR in the Postwar Period," July 9, 1946, *ibid.*, ser. CCS 092 USSR (3-27-45), JIS 80/26; MID, "Ability of Potential Enemies to Attack the Continental United States," August 8, 1946, RG 319, P&O 381 (top secret); and JIC, "Soviet Capabilities to Launch Air Attacks against the United Kingdom," November 29, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JIC 375/1. For assessments of the improvements in Soviet air power, see, for example, Patterson to Truman, June 23, 1947, HTL, HSTP, PSF, box 157; JIC, Moscow Embassy, "Soviet Intentions," April 1, 1948, RG 330, box 4, CD2-2-2.

⁴² See, for example, JIS, "Russian Military Capabilities," October 25, 1945; JIS, "Estimate of Soviet Postwar Military Capabilities and Intentions," November 8, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JIS 80/14; JIS, "Military Capabilities of Great Britain and France," November 13, 1945; JWPC, "Military Position of the United States in Light of Russian Policy," January 8, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JWPC 416/1; and Inglis, Memorandum of Information, January 21, 1946, ML, JFP, box 24.

⁴³ JLPC, "Russian Capabilities," November 15, 1945; and MID, "Intelligence Estimate of the World Situation for the Next Five Years," August 21, 1946, RG 319, P&O, 350.05 (top secret). For a contemporary analysis of the Soviet transport network, also see Paul Wohl, "Transport in the Development of Soviet Policy," *Foreign Affairs*, 24 (1946): 466–83.

⁴⁴ JIS, "Soviet Post-War Military Policies and Capabilities," January 15, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JIS 80/24; MID, "Ability of Potential Enemies to Attack the Continental United States," August 8, 1946; and P&O, "Estimate of the Situation Pertaining to the Northeast Approaches to the United States," August 12, 1946, RG 319, P&O, 381 (top secret).

⁴⁵ For the views of Eisenhower and Sherman, see S. W. D., Memorandum for the Record, June 12, 1946; Galambos, *Papers of Dwight David Eisenhower*, 7: 1012–13, 1106–07; Sherman, "Presentation to the President,"

In general, improvements in specific areas of the Soviet military establishment did not mean that overall Soviet capabilities were improving at an alarming rate. In July 1947, the Military Intelligence Division concluded, "While there has been a slight overall improvement in the Soviet war potential, Soviet strength for total war is not sufficiently great to make a military attack against the United States anything but a most hazardous gamble." This view prevailed in 1946 and 1947, even though the American nuclear arsenal was extremely small and the American strategic bombing force of limited size. In the spring of 1948 the Joint Intelligence Committee at the American embassy in Moscow explained why the United States ultimately would emerge victorious should a war erupt in the immediate future. The Soviets could not win because of their "inability to carry the war to U.S. territory. After the occupation of Europe, the U.S.S.R. would be forced to assume the defensive and await attacks by U.S. forces which should succeed primarily because of the ability of the U.S. to outproduce the U.S.S.R. in materials of war."⁴⁶

Awareness of Soviet economic shortcomings played a key role in the American interpretation of Soviet capabilities. Intelligence reports predicted that Soviet leaders would invest a disproportionate share of Russian resources in capital goods industries. But, even if such Herculean efforts enjoyed some success, the Soviets still would not reach the pre-World War II levels of the United States within fifteen to twenty years. Technologically, the Soviets were behind in the critical areas of aircraft manufacturing, electronics, and oil refining. And, despite Russia's concerted attempts to catch up and to surpass the United States, American intelligence experts soon started reporting that Soviet reconstruction was lagging behind Soviet ambitions, especially in the electronics, transportation, aircraft, construction machinery, nonferrous metals, and shipping industries. Accordingly, throughout the years 1945-48 American military analysts and intelligence experts believed that Soviet transportation bottlenecks, industrial shortcomings, technological backwardness, and agricultural problems would discourage military adventurism.⁴⁷

January 14, 1947, NHC, Forrest E. Sherman Papers, box 2; for the views of Lincoln, Glover, Patterson, Forrestal, and others on Soviet logistical problems, see JPS, Minutes of the 249th meeting, May 22, 1946, RG 218, ser. CCS 381 USSR (3-2-46); Glover to Lincoln and Kissner, June 24, 1947, NHC, SPD, ser. 4, box 86; Louis Denfeld, Memorandum, March 29, 1948, *ibid.*, central files, 1948, A16-3 (5); and Millis, *The Forrestal Diaries*, 272. For assessments of Soviet air power, see, for example, JIS, "Estimate of Soviet Postwar Military Capabilities and Intentions," November 8, 1945; JIC, "Soviet Capabilities to Launch Air Attacks against the United Kingdom," November 29, 1946; Office of Naval Intelligence [hereafter, ONI], "A Study of B-29 Airfields with a Capacity in Excess of 120,000 Pounds" [Spring 1948], NHC, General Board 425 (ser. 315); General Board, "National Security and Navy," Enclosure B, June 25, 1945, *ibid.*, page 16; Forrestal and Clarence Cannon, Excerpt of Conversation, April 9, 1948, ML, JFP, box 48; Inglis to Op-30, December 1, 1948, NHC, SPD, central files, 1948, A8; and Robert Lovett, Diary Entry, December 16, 1947, New York Historical Society, Robert Lovett MS Diaries. For a recent assessment of Soviet conventional strength, see Matthew A. Evangelista, "Stalin's Postwar Army Reappraised," *International Security*, 7 (1982-83): 110-38.

⁴⁶ MID, "Estimate of the Possibility of War between the United States and the USSR Today from a Comparison with the Situation as It Existed in September 1946," July 21, 1947, RG 319, P&O, 350.05 (top secret); and JIC, Moscow Embassy, "Soviet Intentions," April 1, 1948.

⁴⁷ For assessments of the interrelationships between the state of the Soviet economy and Soviet military capabilities, see, for example, JIS, "Postwar Economic Policies and Capabilities of the USSR," November 1, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JIS 80/12; JIS, "Soviet Postwar Economic Capabilities and Policies," January 8, 1946, *ibid.*, JIS 80/22; JIS, "Soviet Post-War Military Policies and Capabilities," January 15, 1946; W. B. Shockley, "Relative Technological Achievements in Weapons Characteristics in USSR and USA," January 30, 1946, RG 107, RPPP, safe file, box 6; MID, "Ability of Potential Enemies to Attack the United States," August 8, 1946; U.S. Military Attache (Moscow) to Chamberlin, March 21, 1947, NHC, Operations

IF AMERICAN DEFENSE OFFICIALS DID NOT EXPECT a Soviet military attack, why, then, were they so fearful of losing control of Eurasia? The answer rests less in American assessments of Soviet military capabilities and short-term military intentions than in appraisals of economic and political conditions throughout Europe and Asia. Army officials in particular, because of their occupation roles in Germany, Japan, Austria, and Korea, were aware of the postwar plight of these areas. Key military men—Generals Clay, Douglas MacArthur, John Hildring, and Oliver P. Echols and Colonel Charles H. Bonesteel—became alarmed by the prospects of famine, disease, anarchy, and revolution. They recognized that communist parties could exploit the distress and that the Russians could capitalize upon it to spread Soviet influence. As early as June 1945, Rear Admiral Ellery Stone, the American commissioner in Italy, wrote that wartime devastation had created fertile soil for the growth of communism in Italy and the enlargement of the Soviet sphere. MacArthur also feared that, if the Japanese economy remained emasculated and reforms were not undertaken, communism would spread. Clay, too, was acutely aware that German communists were depicting themselves and their beliefs as their country's only hope of salvation. In the spring of 1946 military planners, working on contingency plans for the emergency withdrawal of American troops from Germany, should war with Russia unexpectedly occur, also took note of the economic turmoil and political instability in neighboring countries, especially France. Sensitivity to the geopolitical dimensions of the socioeconomic crisis of the postwar era impelled Chief of Staff Eisenhower to give high priority in the army budget to assistance for occupied areas.⁴⁸

Civilian officials in the War, Navy, and State departments shared these concerns. In the autumn of 1945, McCloy warned Patterson that the stakes in Germany were immense and economic recovery had to be expedited. During the first half of 1946 Secretary Patterson and Assistant Secretary Peterson continually pressed the State Department to tackle the problems beleaguering occupation authorities in Germany and pleaded for State Department support and assistance in getting the Truman administration to provide additional relief to the devastated areas of Europe. On Peterson's urging, Acheson wrote Truman in April 1946, "We have now reached the most critical period of the world food crisis. We must either immediately greatly increase the exports of grain from the United States or expect general disorder and political upheaval to develop in [most of Eurasia]."⁴⁹ Forrestal had already pressed

Division, ser. 2 (secret and under), box 33, EF 61; JIC, "Soviet Military Objectives and Capabilities," October 27, 1947; and JIC, Moscow Embassy, "Soviet Intentions," April 1, 1948.

⁴⁸ Stone to Admiral H. R. Stark, June 25, 1945, NHC, double zero files, 1942–47, folder 23. For MacArthur's view, see J. W. Dower, *Empire and Aftermath: Yoshida Shigeru and the Japanese Experience* (Cambridge, Mass., 1979), 292–303; for the situation in Germany, see Patterson to Byrnes, February 25, 1946, RG 107, RPPP, general decimal file, box 8; materials in RG 165, ser. ABC 387 Germany (12-18-43), sects. 4D, 4E; Smith, *Papers of General Lucius D. Clay*, 1: 165–66, 184, 187–89, 196–98, 201–02, 207–08, 217; Galambos, *Papers of Dwight David Eisenhower*, 7: 892; and, for the relationship between the situation in France and American war planning, see, for example, JPS, Minutes of the 249th and 250th meetings, May 22, 29, 1946, RG 218, ser. CCS 381 USSR (3-2-46). For evolving war plans in the Pincher series, see *ibid.*, sects. 1, 2; also see Forrestal, "French Situation," May 6, 1946, ML, JFP, box 20; and *FRUS*, 1946, 5: 434–40; and, for Eisenhower's concern, see Galambos, *Papers of Dwight David Eisenhower*, 8: 1516–20.

⁴⁹ Acheson to Truman, April 30, 1946, RG 107, HCPP, general subject file, box 1. Also see McCloy to Patterson, November 24, 1945, *ibid.*, RPPP, safe file, box 4. For pressure on the State Department, see Patterson

for a reassessment of occupation policies in Germany and Japan. In May, Clay suspended reparation payments in order to effect an accord on German economic unity. In June, Patterson began to support the merger of the American and British zones. The man most responsible for this latter undertaking was William Draper, Forrestal's former partner in Dillon, Read, and Co., and Clay's chief economic assistant. Draper firmly believed that "economic collapse in either [France or Germany] with probable political break-down and rise of communism would seriously threaten American objectives in Europe and in the world."⁵⁰

American defense officials, military analysts, and intelligence officers were extremely sensitive to the political ferment, social turmoil, and economic upheaval throughout postwar Europe and Asia. In their initial postwar studies, the Joint Chiefs of Staff carefully noted the multiplicity of problems that could breed conflict and provide opportunities for Soviet expansion. In the spring of 1946 army planners, including General Lincoln, were keenly aware that conflict was most likely to arise from local disputes (for example, in Venezia-Giulia) or from indigenous unrest (for example, in France), perhaps even against the will of Moscow. A key War Department document submitted to the State-War-Navy Coordinating Committee in April 1946 skirted the issue of Soviet military capabilities and argued that the Soviet Union's strength emanated from totalitarian control over its satellites, from local communist parties, and from worldwide chaotic political and economic conditions. In October 1946 the Joint Planning Staff stressed that for the next ten years the major factor influencing world political developments would be the East-West ideological conflict taking place in an impoverished and strife-torn Europe and a vacuum of indigenous power in Asia. "The greatest danger to the security of the United States," the CIA concluded in mid-1947, "is the possibility of economic collapse in Western Europe and the consequent accession to power of Communist elements."⁵¹

to Byrnes, December 10, 1945, RG 165, Civil Affairs Division [hereafter, CAD], ser. 014 Germany; Patterson to Byrnes, February 25, 1946; OPD and CAD, "Analysis of Certain Political Problems Confronting Military Occupation Authorities in Germany," April 10, 1946, RG 107, HCPP, 091 Germany (classified); and "Combined Food Board" file, spring 1946, *ibid.*, HCPP, general subject file, box 1.

⁵⁰ William Draper, Memorandum [early 1947], RG 107, HCPP, 091 Germany (classified); and Forrestal to Acheson, January 14, 1946, ML, JFP, box 68. For Clay's initiative, see Smith, *Papers of General Lucius D. Clay*, 1: 203-04, 213-14, 218-23; John F. Gimbel, *The American Occupation of Germany: Politics and the Military, 1945-1949* (Stanford, 1968), 35-91; John H. Backer, *The Decision to Divide Germany: American Foreign Policy in Transition* (Durham, N.C., 1978), 137-48; and Bruce Kuklick, *American Policy and the Division of Germany: The Clash with Russia over Reparations* (Ithaca, N.Y., 1972), 205-35. For Patterson's concerns and his support of Bizonia, see Patterson to Byrnes, June 11, 1946, RG 107, HCPP, 091 Germany (classified); Patterson to Truman, November 20, 1946, *ibid.*, RPPP, safe file, box 4; Minutes of the War Council meeting, December 5, 1946, *ibid.*, box 7; and Patterson to Palmer Hoyt, December 27, 1946, *ibid.*, box 4. For the merger of the zones, also see *FRUS, 1946*, 5: 579-659; Smith, *Papers of General Lucius D. Clay*, 1: 245, 248-49; and, for Draper's importance, also see Carolyn Eisenberg, "U.S. Social Policy in Post-War Germany: The Conservative Restoration," paper delivered at the Seventy-Fourth Annual Meeting of the Organization of American Historians, held in April 1981, in Detroit.

⁵¹ CIA, "Review of the World Situation as It Relates to the Security of the United States," September 26, 1947. Also see, for example, JCS, "Strategic Concept and Plan for the Employment of United States Armed Forces," Appendix A, September 19, 1945; JPS, Minutes of the 249th and 250th meetings; Lincoln to Wood, May 22, 1946, RG 165, ser. ABC 381 (9-1-45); [Giffin (?)] "U.S. Policy with Respect to Russia" [early April 1946], *ibid.*, ser. ABC 336 (8-22-43); JPS, "Estimate of Probable Developments in the World Political Situation up to 1956," October 31, 1946, RG 218, ser. CCS 092 (10-9-46), JPS 814/1; MID, "World Political Developments Affecting the Security of the United States during the Next Ten Years," April 14, 1947, RG 319, P&O, 350.05 (top secret).

In brief, during 1946 and 1947, defense officials witnessed a dramatic unraveling of the geopolitical foundations and socioeconomic structure of international affairs. Britain's economic weakness and withdrawal from the eastern Mediterranean, India's independence movement, civil war in China, nationalist insurgencies in Indo-China and the Dutch East Indies, Zionist claims to Palestine and Arab resentment, German and Japanese economic paralysis, communist inroads in France and Italy—all were ominous developments. Defense officials recognized that the Soviet Union had not created these circumstances but believed that Soviet leaders would exploit them. Should communists take power, even without direct Russian intervention, the Soviet Union, it was assumed, would gain predominant control of the resources of these areas because of the postulated subservience of communist parties everywhere to the Kremlin. Should nationalist uprisings persist, communists seize power in underdeveloped countries, and Arabs revolt against American support of a Jewish state, the petroleum and raw materials of critical areas might be denied the West. The imminent possibility existed that, even without Soviet military aggression, the resources of Eurasia could fall under Russian control. With these resources, the Soviet Union would be able to overcome its chronic economic weaknesses, achieve defense in depth, and challenge American power—perhaps even by military force.⁵²

IN THIS FRIGHTENING POSTWAR ENVIRONMENT American assessments of Soviet long-term intentions were transformed. When World War II ended, military planners initially looked upon Soviet aims in foreign affairs as arising from the Kremlin's view of power politics, Soviet strategic imperatives, historical Russian ambitions, and Soviet reactions to moves by the United States and Great Britain. American intelligence analysts and strategic planners most frequently discussed Soviet actions in Eastern Europe, the Balkans, the Near East, and Manchuria as efforts to establish an effective security system. Despite enormous Soviet gains during the war, many assessments noted that, in fact, the Soviets had not yet achieved a safe security zone, especially on their southern periphery. While Forrestal, Deane, and most of the planners in the army's Operations Division possessed a skeptical, perhaps even sinister, view of Soviet intentions, the still prevailing outlook at the end of 1945 was to dismiss the role of ideology in Soviet foreign policy yet emphasize Soviet distrust of foreigners; to stress Soviet expansionism but acknowledge the possibility of accommodation; to abhor Soviet domination of Eastern Europe but discuss Soviet policies elsewhere in terms of power and influence; and to dwell upon the Soviet preoccupation with security yet acknowledge doubt about ultimate Soviet intentions.⁵³

⁵² See, for example, JIS, "Soviet Postwar Economic Capabilities," January 8, 1946; MID, "Intelligence Estimate of the World Situation," June 25, 1946; JCS, "Presidential Request for Certain Facts and Information Regarding the Soviet Union," July 25, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JCS 1696; H. D. Riley to Op-30, February 7, 1947, NHC, SPD, ser. 5, box 111, A16-3 (5); MID, "Capabilities (Ground) and Intentions of the USSR for Overrunning Northern and Western Europe," February 28, 1947; P&O, "Strategic Study of Western and Northern Europe," May 21, 1947, RG 319, P&O, 092 (top secret); and Wooldridge to the General Board, April 30, 1948.

⁵³ For assessments of Soviet intentions, see, for example, JIC, "Estimate of Soviet Post-War Capabilities and Intentions," February 2, 1945, RG 218, ser. CCS 000.1 USSR (10-2-44), JIC 250/2; John S. Wise to Hull, April

This orientation changed rapidly during 1946. In January, the Joint War Plans Committee observed that "the long-term objective [of the Soviet Union] is deemed to be establishment of predominant influence over the Eurasian land mass and the strategic approaches thereto." Reports of the new military attaché in Moscow went further, claiming that "the ultimate aim of Soviet foreign policy seems to be the dominance of Soviet influence throughout the world" and "the final aim . . . is the destruction of the capitalist system." Soon thereafter, Kennan's "long telegram" was widely distributed among defense officials, on whom it had considerable impact. Particularly suggestive was his view that Soviet leaders needed the theme of capitalist encirclement to justify their autocratic rule. Also influential were Kennan's convictions that the Soviet leaders aimed to shatter the international authority of the United States and were beyond reason and conciliation.⁵⁴

During the spring and summer of 1946, defense officials found these notions persuasive as an interpretation of Soviet intentions because of the volatile international situation, the revival of ideological fervor within the Soviet Union, and the domestic political atmosphere and legislative constraints in the United States. President Truman wished to stop "babying the Soviets," and his predilection for a tougher posture probably led his subordinates to be less inclined to give the Soviets the benefit of any doubt when assessing Russian intentions.⁵⁵ Forrestal believed the Soviet communist threat had become more serious than the Nazi challenge of the 1930s; General John E. Hull, director of the Operations Division, asserted that the Soviets were "constitutionally incapable of being conciliated"; and Clark Clifford and George Elsey considered Soviet fears "absurd." A key subcommittee of the State-War-Navy Coordinating Committee declared that Soviet suspicions were "not susceptible of removal," and in July 1946 the Joint Chiefs of Staff declared the Soviet objective to be "world domination." By late 1946 it was commonplace for intelligence reports and military assessments to state, without any real analysis, that the "ultimate aim of Soviet foreign policy is Russian domination of a communist

3, 1945, RG 319, P&O, 350.05, State Department red file (top secret); JSSC to JCS, April 5, 1945, RG 218, ser. CCS 092 USSR (3-27-45); Deane, "Revision of Policy with Relation to Russia," April 16, 1945 (JCS 1313); Secretary of War to Secretary of State [early July 1945], RG 165, ser. ABC 093 Kiel (7-6-45); Marshall to McCloy, July 3, 1945, *ibid.*, OPD 336 (top secret); OPD, "Soviet Intentions," July 6, 1945, *ibid.*, ser. ABC 092 USSR (11-15-44); JCS, "United States Policy concerning the Dardanelles" [July 1945]; JCS, "Military Position of the United States in the Light of Russian Policy," October 8, 1945, RG 218, ser. CCS 092 USSR (3-27-45), JCS 1545; JIS, "Soviet Postwar Foreign Policy," October 25, 1945, *ibid.*, JIS 80/9; JIS, "Russian Military Capabilities," October 25, 1945; Ritchie, "Report of the United States Mission to Moscow, 18 October 1943 to 31 October 1945" [October 31, 1945], RG 165, OPD 336 (top secret); ONI, "Basic Factors in World Situation," December 1945, NHC, SPD, ser. 5, box 106, A8; JIS, "Capabilities and Intentions of the USSR in the Post-War Period," January 2, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JIS 80/20; Forrestal, Notes for remarks to the Harvard Club, January 18, 1946, JL, JFP, box 29; Inglis, Memorandum of Information, January 21, 1946; Stoler, "Continentalism to Globalism," 315-21; and Sherry, *Preparing for the Next War*, 159-90.

⁵⁴ JWPC, "Military Position of the United States in Light of Russian Policy," January 8, 1946; and U.S. Military Attache (Moscow), "Estimate of the Situation as of February 1," February 18, 1946, RG 165, ser. ABC 381 Germany (1-29-43). For Kennan's telegram, see *FRUS*, 1946, 4: 696-709; and, for the distribution of Kennan's telegram, see R. L. Vittrup, Memorandum for Craig, February 26, 1946, RG 107, RPPP, safe file, box 5; Vittrup to Lincoln, March 1, 1946, RG 319, P&O, 350.05, State Department red file (top secret); and Bruce Hopper to Kennan, March 29, 1946, ML, GFKP, box 28.

⁵⁵ Robert L. Messer, *The End of an Alliance: James F. Byrnes, Roosevelt, Truman, and the Origins of the Cold War* (Chapel Hill, N.C., 1982), 152-94, and "Paths Not Taken," 297-319.

world.”⁵⁶ There was, of course, plentiful evidence for this appraisal of Soviet ambitions—the Soviet consolidation of a sphere of influence in Eastern Europe; the incendiary situation in Venezia Giulia; Soviet violation of the agreement to withdraw troops from Iran; Soviet relinquishment of Japanese arms to the Chinese communists; the Soviet mode of extracting reparations from the Russian zone in Germany; Soviet diplomatic overtures for bases in the Dardanelles, Tripolitania, and the Dodecanese; Soviet requests for a role in the occupation of Japan; and the Kremlin’s renewed emphasis on Marxist-Leninist doctrine, the vulnerability of capitalist economies, and the inevitability of conflict.

Yet these assessments did not seriously grapple with contradictory evidence. While emphasizing Soviet military capabilities, strategic ambitions, and diplomatic intransigence, reports like the Clifford-Elsey memorandum of September 1946 and the Joint Chiefs of Staff report 1696 (upon which the Clifford-Elsey memorandum heavily relied) disregarded numerous signs of Soviet weakness, moderation, and circumspection. During 1946 and 1947 intelligence analysts described the withdrawal of Russian troops from northern Norway, Manchuria, Bornholm, and Iran (from the latter under pressure, of course). Numerous intelligence sources reported the reduction of Russian troops in Eastern Europe and the extensive demobilization going on within the Soviet Union. In October 1947 the Joint Intelligence Committee forecast a Soviet army troop strength during 1948 and 1949 of less than two million men. Soviet military expenditures appeared to moderate. Other reports dealt with the inadequacies of Soviet transportation and bridging equipment for the conduct of offensive operations in Eastern Europe. And, as already noted, assessments of the Soviet economy revealed persistent problems likely to restrict Soviet adventurism.⁵⁷

Experience suggested that the Soviet Union was by no means uniformly hostile

⁵⁶ Forrestal to Clarence Dillon, April 11, 1946, ML, JFP, box 11; Hull to Theater Commanders, March 21, 1946, RG 165, ser. ABC 336 (8-22-43); for the Clifford-Elsey viewpoint, see Krock, *Memoirs: Sixty Years on the Firing Line*, 428; and SWNCC, “Resume of Soviet Capabilities and Possible Intentions,” August 29, 1946, NHC, SPD, ser. 5, box 106, A8. For the SWNCC estimate, see JCS, “Political Estimate of Soviet Policy for Use in Connection with Military Studies,” April 5, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JCS 1641/4; and JCS “Presidential Request for Certain Facts and Information Regarding the Soviet Union,” July 25, 1946. Some of the most thoughtful studies on Soviet intentions, like that of the Joint Intelligence Staff in early January 1946 (JIS 80/20), were withdrawn from consideration. See the evolution of studies and reports in RG 218, ser. CCS 092 USSR (3-27-45), sects. 5–7.

⁵⁷ For the withdrawal of Soviet troops, see, for example, MID, “Soviet Intentions and Capabilities in Scandinavia as of 1 July 1946,” April 25, 1946, RG 319, P&O, 350.05 (top secret); and [Giffin (?)] “U.S. Policy with Respect to Russia” [early April 1946]. For reports on reductions of Russian troops in Eastern Europe and demobilization within the Soviet Union, see MID, “Review of Europe, Russia, and the Middle East,” December 26, 1945, RG 165, OPD, 350.05 (top secret); Carl Espe, weekly calculations of Soviet troops, May–September 1946, NHC, SPD, ser. 5, box 106, A8; MID, “Soviet Capabilities in Germany and West Europe,” December 26, 1946, RG 319, P&O, 350.05 (top secret); JIC, “Movement of Russian Troops Outside of USSR except in the Far East,” December 31, 1946, RG 218, ser. CCS 092 USSR (3-27-45), JIC Memorandum of Information no. 237; MID, “Estimate of the Possibility of War,” July 21, 1947; and JIC, “Soviet Military Objectives and Capabilities,” October 27, 1947. For references to Soviet military expenditures, see Patterson to Julius Adler, November 2, 1946, RG 107, RPPP, safe file, box 5; and Abram Bergson, “Russian Defense Expenditures,” *Foreign Affairs*, 26 (1948): 373–76. And, for assessments of the Soviet transport system, see R. F. Ennis, Memorandum for the P&O Division, June 24, 1946, RG 165, ser. ABC 336 (8-22-43); U.S. Military Attache (Moscow) to Chamberlain, March 21, 1947; Op-32 to the General Board, April 28, 1948, NHC, General Board 425 (ser. 315); and Wohl, “Transport in the Development of Soviet Policy,” 475–76, 483.

or unwilling to negotiate with the United States. In April 1946, a few days after a State-War-Navy subcommittee issued an alarming political estimate of Soviet policy (for use in American military estimates), Ambassador Smith reminded the State Department that the Soviet press was not unalterably critical of the United States, that the Russians had withdrawn from Bornholm, that Stalin had given a moderate speech on the United Nations, and that Soviet demobilization continued apace. The next month General Lincoln, who had accompanied Byrnes to Paris for the meeting of the council of foreign ministers, acknowledged that the Soviets had been willing to make numerous concessions regarding Tripolitania, the Dodecanese, and Italian reparations. In the spring of 1946, General Echols, General Clay, and Secretary Patterson again maintained that the French constituted the major impediment to an agreement on united control of Germany. At the same time the Soviets ceased pressing for territorial adjustments with Turkey. After the diplomatic exchanges over the Dardanelles in the late summer of 1946 the Soviets did not again ask for either a revision of the Montreux Convention or the acquisition of bases in the Dardanelles. In early 1947 central intelligence delineated more than a half-dozen instances of Soviet moderation or concessions. In April the Military Intelligence Division noted that the Soviets had limited their involvement in the Middle East, diminished their ideological rhetoric, and given only moderate support to Chinese communists. In the months preceding the Truman Doctrine, Soviet behavior—as noted by American military officials and intelligence analysts—hardly justified the inflammatory rhetoric Acheson and Truman used to secure congressional support for aid to Greece and Turkey. Perhaps this is why General Marshall, as secretary of state, refrained from such language himself and preferred to focus on the socioeconomic aspects of the unfolding crisis.⁵⁸

In their overall assessments of Soviet long-term intentions, however, military planners dismissed all evidence of Soviet moderation, circumspection, and restraint. In fact, as 1946 progressed, these planners seemed to spend less time analyzing Soviet intentions and more time estimating Soviet capabilities.⁵⁹ Having accepted the notion that the two powers were locked in an ideological struggle of

⁵⁸ For the SWNCC estimate, see JCS, "Political Estimate of Soviet Policy," April 5, 1946; for Smith's despatch, see Smith to the Secretary of State, April 11, 1946, RG 165, Records of the Chief of Staff, 091 Russia; and, for Soviet negotiating concessions, see Lincoln, Memorandum for the Chief of Staff, May 20, 1946, USMA, GLP, War Dept. files; James F. Byrnes, *Speaking Frankly* (New York, 1947), 129–37; Patricia Dawson Ward, *The Threat of Peace: James F. Byrnes and the Council of Foreign Ministers* (Kent, Ohio, 1979), 95–102. For the situation in Germany, see OPD and CAD, "Analysis of Certain Political Problems Confronting Military Occupation Authorities in Germany," April 10, 1946, RG 107, HCPP, 091 Germany (classified); Patterson to Truman, June 11, 1946, RG 165, Records of the Chief of Staff, 091 Germany. For Clay's references to French obstructionism, see, for example, Smith, *Papers of General Lucius D. Clay*, 1: 84–85, 88–89, 151–52, 189–90, 212–17, 235–36; for American perceptions of the situation in Turkey, see Melvyn P. Leffler, "Strategy, Diplomacy, and the Cold War: The United States, Turkey, and N.A.T.O.," paper delivered at the Seventy-Fifth Annual Meeting of the Organization of American Historians, held in April 1983, in Cincinnati; for overall intelligence assessments, see Central Intelligence Group [hereafter, CIG], "Revised Soviet Tactics in International Affairs," January 6, 1947, HTL, HSTP, PSF, box 254; MID, "World Political Developments Affecting the Security of the United States during the Next Ten Years," April 14, 1947; and Walter E. Todd to the Director of P&O, April 25, 1947, RG 319, P&O, 350.05 (top secret); for background on the Truman Doctrine, see Joseph Jones, *The Fifteen Weeks* (New York, 1955), esp. 138–70; and, for Marshall's emphasis on the economic roots of the European crisis, see *ibid.*, 203–06, 220–24; and Charles Bohlen, *The Transformation of American Foreign Policy* (New York, 1969), 87–89.

⁵⁹ Both the quantity and the quality of JCS studies on Soviet intentions seem to have declined during 1946. In "Military Position of the United States in Light of Russian Policy" (January 8, 1946), strategic planners of the Joint War Plans Committee maintained that it was more important to focus on Soviet capabilities than on Soviet

indefinite duration and conscious of the rapid demobilization of American forces and the constraints on American defense expenditures, they no longer explored ways of accommodating a potential adversary's legitimate strategic requirements or pondered how American initiatives might influence the Soviet Union's definition of its objectives.⁶⁰ Information not confirming prevailing assumptions either was ignored in overall assessments of Soviet intentions or was used to illustrate that the Soviets were shifting tactics but not altering objectives. Reflective of the emerging mentality was a report from the Joint Chiefs of Staff to the president in July 1946 that deleted sections from previous studies that had outlined Soviet weaknesses. A memorandum sent by Secretary Patterson to the president at the same time was designed by General Lauris Norstad, director of the War Department's Plans and Operations Division, to answer questions about relations with the Soviet Union "without ambiguity." Truman, Clark Clifford observed many years later, liked things in black and white.⁶¹

DURING 1946 AND EARLY 1947, the conjunction of Soviet ideological fervor and socioeconomic turmoil throughout Eurasia contributed to the growth of a myopic view of Soviet long-term policy objectives and to enormous apprehension lest the Soviet Union gain control of all the resources of Eurasia, thereby endangering the national security of the United States. American assessments of Soviet short-term military intentions had not altered; Soviet military capabilities had not significantly increased, and Soviet foreign policy positions had not greatly shifted. But defense officials were acutely aware of America's own rapidly diminishing capabilities, of Britain's declining military strength, of the appeal of communist doctrine to most of the underdeveloped world, and of the opportunities open to communist parties throughout most of Eurasia as a result of prevailing socioeconomic conditions. War Department papers, studies of the joint chiefs, and intelligence analyses repeatedly described the restiveness of colonial peoples that had sapped British and French strength, the opportunities for communist parties in France, Italy, and even Spain to capitalize upon indigenous conditions, and the ability of the Chinese communists to defeat the nationalists and make the resources and manpower of Manchuria and North China available to the Soviet Union. In this turbulent international arena, the survival of liberal ideals and capitalist institutions was anything but assured. "We

intentions. During a key discussion at the White House, Admiral Leahy also was eager to dismiss abstract evaluations of Russian psychology and to focus on Russian capabilities; S. W. D., Memorandum for the Record, June 12, 1946. My assessment of the quality of JCS studies is based primarily on my analysis of the materials in RG 218, ser. CCS 092 USSR (3-27-45); ser. CCS 381 USSR (3-2-46); RG 319, P&O, 350.05 (top secret); and NHC, SPD, central files, 1946-48, A8.

⁶⁰ During 1946 it became a fundamental tenet of American policy makers that Soviet policy objectives were a function of developments within the Soviet Union and not related to American actions. See, for example, Kennan's "long telegram," in *FRUS*, 1946, 4: 696-709; JCS, "Political Estimate of Soviet Policy," April 5, 1946; JCS, "Presidential Request," July 25, 1946; and the Clifford/Elsey memorandum, in Krock, *Memoirs*, esp. 427-36.

⁶¹ For Norstad's comment, see Norstad, Memorandum, July 25, 1946, RG 319, P&O, 092 (top secret). For references to shifting tactics and constant objectives, see Vandenberg, Memorandum for the President, September 27, 1946, HTL, HSTP, PSF, box 249; CIG, "Revised Soviet Tactics," January 6, 1947; and, for the JCS report to the president, compare JCS 1696 with JIC 250/12. Both studies may be found in RG 218, ser. CCS 092 USSR (3-27-45). For Clifford's recollection, Clark Clifford, HTL, oral history, 170.

could point to the economic benefits of Capitalism,” commented one important War Department paper in April 1946, “but these benefits are concentrated rather than widespread, and, at present, are genuinely suspect throughout Europe and in many other parts of the world.”⁶²

In this environment, there was indeed no room for ambiguity or compromise. Action was imperative—action aimed at safeguarding those areas of Eurasia not already within the Soviet sphere. Even before Kennan’s “long telegram” arrived in Washington the joint chiefs adopted the position that “collaboration with the Soviet Union should stop short not only of compromise of principle but also of expansion of Russian influence in Europe and in the Far East.”⁶³ During the spring and summer of 1946, General Lincoln and Admiral Richard L. Conolly, commander of American naval forces in the eastern Atlantic and Mediterranean, worked tirelessly to stiffen Byrnes’s views, avert American diplomatic concessions, and put the squeeze on the Russians.⁶⁴ “The United States,” army planners explained, “must be able to prevent, by force if necessary, Russian domination of either Europe or Asia to the extent that the resources of either continent could be mobilized against the United States.” Which countries in Eurasia were worth fighting over remained unclear during 1946. But army and navy officials as well as the joint chiefs advocated a far-reaching program of foreign economic assistance coupled with the refurbishment of American military forces.⁶⁵

During late 1946 and early 1947, the Truman administration assumed the initiative by creating German Bizonia, providing military assistance to Greece and Turkey, allocating massive economic aid to Western Europe, and reassessing

⁶² [Giffin] “U.S. Policy with Respect to Russia” [early April 1946]. Also see Giffin, Draft of Proposed Comments for Assistant Secretary of War on “Foreign Policy,” [early February 1946]; MID, “Intelligence Estimate,” June 25, 1946; JPS, “Estimate of Probable Developments in the World Political Situation,” October 31, 1946, RG 218, ser. CCS 092 (10-9-46), JPS 814/1; Special Ad Hoc Committee of SWNCC, “Study on U.S. Assistance to France,” April 9, 1947, RG 165, ser. ABC 400.336 France (3-20-47); MID, “World Political Developments,” April 14, 1947; JWPC, “The Soviet Threat against the Iberian Peninsula and the Means Required to Meet It,” May 8, 1947, RG 218, ser. CCS 381 USSR (3-2-46), JWPC 465/1; and CIA, “Review of the World Situation,” September 26, 1947. With regard to China, the Joint Chiefs of Staff stressed the importance of Soviet aid to Chinese communist forces in late 1945. But both naval and army intelligence were fully cognizant of the corruption and ineptness of nationalist forces and of the indigenous appeal of the Chinese communist party. The important point, from the perspective of American defense officials, was that Chinese communist victories would offer the Soviets control over critical resources and enable them to achieve greater defense in depth in parts of Asiatic Russia. See, for example, Patterson, Notes on Cabinet Meeting, August 2, 1946, RG 107, RPPP, safe file, box 2; Minutes of the meetings of the Secretaries of State, War, and Navy, September 11, 1946, February 12, 1947, June 20, 1947, *ibid.*, box 3; Lincoln, Proposed Memorandum for the Secretaries of War and Navy [September 1946], NHC, SPD, ser. 12, box 158, C2 (4); Richard M. Phillips, Memorandum, September 6, 1946, *ibid.*; Charles J. Rend, Memorandum of Information, June 3, 1947, *ibid.*, ser. 5, box 110, A8; Nimitz to JCS, June 9, 1947, *ibid.*, box 109, A1; JWPC, “Moonrise,” June 16, 1947, RG 218, ser. CCS 381 USSR (3-2-46), JWPC 476/1; MID, “Soviet Influence in China,” June 16, 1947, RG 319, P&O, 350.05 (top secret); and SWNCC, “United States Policy Toward China,” June 11, 1947, *FRUS*, 1947, 7: 838–48.

⁶³ JCS, “Foreign Policy of the United States,” February 10, 1946.

⁶⁴ Lincoln to Hull [April 1946], RG 59, Office of European Affairs, box 17; Lincoln, Memorandum for the Record, April 16, 1946; Lincoln to Hull, April 16, 1946, RG 165, ser. ABC 092 USSR (11-15-44); Lincoln to Cohen, June 22, 1946, *ibid.*, ABC 381 (9-1-45); Richard L. Conolly, oral history (Columbia, 1960), 293–304; Lincoln, Memorandum for Chief of Staff, May 20, 1946; and Lincoln, Memorandum for Norstad, July 23, 1946, USMA, GLP, War Department files.

⁶⁵ Giffin, “Draft of Proposed Comments” [early February 1946]. Also see, for example, JCS, “Foreign Policy of the United States,” February 10, 1946; [Giffin] “U.S. Policy with Respect to Russia” [early April 1946]; JCS, “Political Estimate of Soviet Policy,” April 5, 1946; and Sherman, Memorandum for Forrestal, March 17, 1946, ML, JFP, box 24.

economic policy toward Japan. These initiatives were aimed primarily at tackling the internal sources of unrest upon which communist parties capitalized and at rehabilitating the industrial heartlands of Eurasia. American defense officials supported these actions and acquiesced in the decision to give priority to economic aid rather than rearmament. Service officers working on foreign assistance programs of the State-War-Navy Coordinating Committee supported economic aid, showed sensitivity to the socioeconomic sources of unrest, and recognized that economic aid was likely to be the most efficacious means of preserving a favorable balance of power in Eurasia.⁶⁶ Because they judged American military power to be superior and war to be unlikely, Forrestal, Lovett, and Webb insisted that military spending not interfere with the implementation of the Marshall Plan, rehabilitation of Germany, and revival of Japan. "In the necessarily delicate apportioning of our available resources," wrote Assistant Secretary of War Peterson, "the time element permits present emphasis on strengthening the economic and social dikes against Soviet communism rather than upon preparing for a possibly eventual, but not yet inevitable, war."⁶⁷

Yet if war should unexpectedly occur, the United States had to have the capability to inflict incalculable damage upon the Soviet Union. Accordingly, Truman shelved (after some serious consideration) proposals for international control of atomic energy. The Baruch Plan, as it evolved in the spring and summer of 1946, was heavily influenced by defense officials and service officers who wished to avoid any significant compromise with the Soviet Union. They sought to perpetuate America's nuclear monopoly as long as possible in order to counterbalance Soviet conventional strength, deter Soviet adventurism, and bolster American negotiating leverage. When negotiations at the United Nations for international control of atomic energy languished for lack of agreement on its implementation, the way was clear for the Truman administration gradually to adopt a strategy based on air power and atomic weapons. This strategy was initially designed to destroy the adversary's will and capability to wage war by annihilating Russian industrial, petroleum, and urban centers.⁶⁸ After completing their study of the

⁶⁶ See, for example, SWNCC, "Policies, Procedures, and Costs of Assistance by the United States to Foreign Countries," April 21, 1947, *FRUS*, 1947, 3: 204–20; *ibid.*, 1: 725–34; JCS, "United States Assistance to Other Countries," *ibid.*, 734–50, 762–63; and Lincoln to Peterson, May 2, 1947, RG 165, ser. ABC 400.336 (3-20-47). Also see the many SWNCC subcommittee reports on individual countries, *ibid.*: Report of the Working Group on Economic Aid to the Special Ad Hoc Committee of the SWNCC, "Foreign Needs for United States Economic Assistance during the Next Three to Five Years" [July 1947], RG 353, box 134; and Report of Rearmament Subcommittee to Special Ad Hoc Committee, July 10, 1947, RG 165, ser. ABC 400.336 (3-20-47).

⁶⁷ Peterson, as quoted in Chief of Staff, Memorandum [July 1947], RG 165, ser. ABC 471.6 Atom (8-17-45). Also see, for example, Lovett diaries, December 16, 1947, January 5, 15, 1948; Baruch to Forrestal, February 7, 1948, ML, JFP, box 78; Forrestal to Baruch, February 10, 1948, *ibid.*; and Excerpt of Phone Conversation between Forrestal and C. E. Wilson, April 2, 1948, *ibid.*, box 48.

⁶⁸ These generalizations are based on the following materials: Stimson, Memorandum for the President, September 11, 1945, RG 107, RPPP, safe file, box 1; Forrestal, Memorandum, September 21, 1945, ML, JFP, box 48; Mathia F. Correa to Forrestal, September 27, 1945, *ibid.*, box 28; Forrestal, Memorandum for the President, October 1, 1945, HTL, HSTP, PSF, box 158; documents in HTL, HSTP, PSF, box 199; RG 165, ser. ABC 471.6 Atom (8-17-45); "Brief of Letters Addressed to Mr. Baruch by Each of the Members of the JCS" [June 1946], Bernard Baruch Papers [hereafter, BBP], ML, box 65; Dennison, Draft Reply to Letter from Mr. Baruch, June 4, 1946, NHC CNO, double zero files, folder 31; and Minutes of the meeting of the Secretaries of State, War, and Navy, January 29, 1947, RG 107, RPPP, safe file, box 3. For the negotiations at the United Nations, see *FRUS*, 1947, 1: 327–614; also see Herken, *Winning Weapon*; Larry G. Gerber, "The Baruch Plan

1946 Bikini atomic tests, the Joint Chiefs of Staff in July 1947 called for an enlargement of the nuclear arsenal. While Truman and Forrestal insisted on limiting military expenditures, government officials moved vigorously to solve problems in the production of plutonium, to improve nuclear cores and assembly devices, and to increase the number of aircraft capable of delivering atomic bombs. After much initial postwar disorganization, the General Advisory Committee to the Atomic Energy Commission could finally report to the president at the end of 1947 that "great progress" had been made in the atomic program. From June 30, 1947, to June 30, 1948, the number of bombs in the stockpile increased from thirteen to fifty. Although at the time of the Berlin crisis the United States was not prepared to launch a strategic air offensive against the Soviet Union, substantial progress had been made in the development of the nation's air-atomic capabilities. By the end of 1948, the United States had at least eighteen nuclear-capable B-50s, four B-36s, and almost three times as many nuclear-capable B-29s as had been available at the end of 1947.⁶⁹

During late 1947 and early 1948, the administration also responded to pleas of the Joint Chiefs of Staff to augment the overseas base system and to acquire bases in closer proximity to the Soviet Union. Negotiations were conducted with the British to gain access to bases in the Middle East and an agreement was concluded for the acquisition of air facilities in Libya. Admiral Conolly made a secret deal with the French to secure air and communication rights and to stockpile oil, aviation gas, and ammunition in North Africa.⁷⁰ Plans also were discussed for postoccupation bases in Japan, and considerable progress was made in refurbishing and constructing airfields in Turkey. During 1948 the Turks also received one hundred eighty F-47 fighter-bombers, thirty B-26 bombers, and eighty-one C-47 cargo planes. The F-47s and B-26s, capable of reaching the vital Ploesti and Baku oil fields, were more likely to be used to slow down a Soviet advance through Turkey or Iran, thereby

and the Origins of the Cold War," *Diplomatic History*, 6 (1982): 69–95; Bernstein, "Quest for Security," 1033–44; and Rosenberg, "Hydrogen Bomb Decision," 66–71.

⁶⁹ For the views of the General Advisory Committee, see Robert Oppenheimer to Truman, December 31, 1947, HTL, HSTP, PSF, box 200; for the views of the JCS, see, for example, JCS, "Guidance on Military Aspects of United States Policy to Be Adopted in Event of Continuing Impasse in Acceptance of International Control of Atomic Energy," July 14, 1947, RG 165, ser. ABC 471.6 Atom (8-17-45), JCS 1764/1; and Leahy to the Secretaries of War and the Navy, August 13, 1947, NHC, CNO, double zero files, 1947, folder 13; and, for the size and quality of the stockpile and the number of nuclear-capable aircraft, see especially David Alan Rosenberg, "U.S. Nuclear Stockpile, 1945 to 1950," *Bulletin of Atomic Scientists*, 38 (1982): 25–30. The number of nuclear-capable B-29s grew with great rapidity at the end of 1948. One memorandum in early 1949 enumerated eighty-three such planes; see O. S. Picher, Memorandum for Colonel Page, February 14, 1949, RG 330, box 126, CD 33-1-4. Both Borowski and Rosenberg have stressed the problems beleaguering the Strategic Air Command until the Korean War, but their work also illustrates the significant changes and improvements that began to occur late in 1947 and especially during 1948. See Borowski, *Hollow Threat*; David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945–1960," *International Security*, 7 (1983): 11–27.

⁷⁰ For negotiations with the British over Middle East strategy and bases, see *FRUS*, 1947, 5: 485–626; and Sullivan to the Acting Secretary of State, September 26, 1947, NHC, SPD, ser. 5, box 110, A14; for facilities in Libya, see, for example, Leahy to the Secretary of Defense, March 18, 1948, RG 319, P&O, 092 (top secret); and *FRUS*, 1948, 3: 906–07; and, for negotiations with the French, see Spaatz to Symington, [October 1947], RG 107, Office of the Assistant Secretary of War for Air, 1947, 090, box 187B; Symington to Spaatz, October 30, 1947, *ibid.*, Wooldridge, Memorandum for Op-09, October 13, 1948, NHC, CNO, double zero files, 1948, box 4 (29); and Wooldridge, Memorandum for Op-09, October 25, 1948, *ibid.*, SPD, central files, 1948, A14. For bases in North Africa, see Forrestal to Truman, January 6, 1948, HTL, HSTP, PSF, box 156; also see *FRUS*, 1948, 1: 603–04, 674–76.

affording time to activate a strategic air offensive from prospective bases in the Cairo-Suez area.⁷¹

Despite these developments, the joint chiefs and military planners grew increasingly uneasy with the budgetary constraints under which they operated. They realized that American initiatives, however necessary, placed the Soviet Union on the defensive, created an incendiary situation, and made war more likely—though still improbable. In July 1947, intelligence analysts in the War Department maintained that the Truman Doctrine and the Marshall Plan had resulted in a more aggressive Soviet attitude toward the United States and had intensified tensions. “These tensions have caused a sharper line of demarcation between West and East tending to magnify the significance of conflicting points of view, and reducing the possibility of agreement on any point.” Intelligence officers understood that the Soviets would perceive American efforts to build strategic highways, construct airfields, and transfer fighter bombers to Turkey as a threat to Soviet security and to the oilfields in the Caucasus. The latter, noted the director of naval intelligence, “lie within easy air striking range of countries on her southern flank, and the Soviet leaders will be particularly sensitive to any political threat from this area, however remote.” Intelligence analysts also recognized that the Soviets would view the Marshall Plan as a threat to Soviet control in Eastern Europe as well as a death-knell to communist attempts to capture power peacefully in Western Europe. And defense officials were well aware that the Soviets would react angrily to plans for currency reform in German Trizonia and to preparations for a West German republic. “The whole Berlin crisis,” army planners informed Eisenhower, “has arisen as a result of . . . actions on the part of the Western Powers.” In sum, the Soviet clampdown in Eastern Europe and the attempt to blockade Berlin did not come as shocks to defense officials, who anticipated hostile and defensive Soviet reactions to American initiatives.⁷²

The real consternation of the Joint Chiefs of Staff and other high-ranking civilian and military officials in the defense agencies stemmed from their growing conviction that the United States was undertaking actions and assuming commitments that now required greater military capabilities. Recognizing that American initiatives, aimed at safeguarding Eurasia from further communist inroads, might

⁷¹ For references to Japanese bases, see, for example, “Discussion of Need of Obtaining Long-Term Rights for a U.S. Naval Operating Base in Japan” (approved by Nimitz) [Autumn 1947], NHC, SPD, ser. 4, box 86; Nimitz to Under Secretary of the Navy, December 12, 1947, *ibid.*, ser. 5, box 110; and Denfeld, Memorandum for Schuyler, February 20, 1948, *ibid.*, central files, 1948, box 245, EF37; for the transfer of planes to Turkey, see Report No. 29, March 12, 1949, RG 59, 867.00/5–1249; and, for the uses of military assistance to Turkey, see Leffler, “The United States, Turkey, and NATO, 1945–52.”

⁷² MID, “Estimate of the Possibility of War,” July 21, 1947, RG 319, P&O, 350.05 (top secret); Op-32 to General Board, April 28, 1948, NHC, General Board 425 (serial 315); and “National Military Establishment Views on Germany” [appended to memorandum for Maddocks], June 30, 1948, RG 319, P&O, 092 (top secret). For the repercussions of the Truman Doctrine and Marshall Plan, see Chamberlin to Chief of Staff, July 9, 1947, RG 165, Records of the Chief of Staff, 091 Greece; and Hillenkoetter, Memorandum for the President, November 7, 1947, HTL, HSTP, PSF, box 249; and, for a similar view in the State Department, see *FRUS*, 1947, 1: 770–75. For prospective Soviet reactions to American assistance to Turkey, also see General Board, “National Security and the Navy,” enclosure D, June 25, 1948; and Conolly to CNO, December 4, 1947, NHC, Operations Division, ser. 1, A4/FF7. For assessments of Soviet reactions to Western initiatives in Germany, also see Hillenkoetter, Memoranda for the President, March 16, 1948, and June 9, 1948, HTL, HSTP, PSF, box 249; CIA, “Possible Program of Future Soviet Moves in Germany,” April 28, 1948, *ibid.*, box 255; and Inglis, Memorandum of Information, April 3, 1948, NHC, Operations Division, ser. 1, box 3.

be perceived as endangering Soviet interests, it was all the more important to be ready for any eventuality. Indeed, to the extent that anxieties about the prospects of war escalated in March and April 1948, these fears did not stem from estimates that the Soviets were planning further aggressive action after the communist seizure of power in Czechoslovakia but from apprehensions that ongoing American initiatives might provoke an attack. On March 14 General S. J. Chamberlin, director of army intelligence, warned the chief of staff that "actions taken by this country in opposition to the spread of Communism . . . may decide the question of the outbreak of war and of its timing." The critical question explicitly faced by the intelligence agencies and by the highest policy makers was whether passage of the Selective Service Act, or of universal military training, or of additional appropriations for the air force, or of a military assistance program to Western European countries, or of a resolution endorsing American support for West European Union would trigger a Soviet attack. Chamberlin judged, for example, that the Soviets would not go to war just to make Europe communist but would resort to war if they felt threatened. The great imponderable, of course, was what, in the Soviet view, would constitute a security threat justifying war.⁷³

Recognizing the need to move ahead with planned initiatives but fearing Soviet countermeasures, the newly formed staff of the National Security Council undertook its first comprehensive assessment of American foreign policy. During March 1948, after consulting with representatives of the army, navy, air force, State Department, CIA, and National Security Resources Board, the National Security Council staff produced NSC 7, "The Position of the United States with Respect to Soviet-Dominated World Communism." This study began with the commonplace assumption that the communist goal was "world conquest." The study then went on to express the omnipresent theme behind all conceptions of American national security in the immediate postwar years. "Between the United States and the USSR there are in Europe and Asia areas of great potential power which if added to the existing strength of the Soviet world would enable the latter to become so superior in manpower, resources, and territory that the prospect for the survival of the United States as a free nation would be slight." Accordingly, the study called, first, for the strengthening of the military potential of the United States and, second, for the arming of the non-Soviet world, particularly Western Europe. Although this staff study was never formally approved, the national security bureaucracy worked during the spring and summer of 1948 for West European unity, military assistance to friendly nations, currency reform in Trizonia, revitalization of the Ruhr, and the founding of the Federal Republic of Germany.⁷⁴

⁷³ For Chamberlin's views, see, for example, Chamberlin, Memorandum to the Chief of Staff, March 14, 1948; and Chamberlin, Memorandum for Wedemeyer, April 14, 1948, RG 319, P&O, 092 (top secret). For the view from Moscow, see JIC, "Soviet Intentions," April 1, 1948 (extracts of this report are printed in *FRUS, 1948*, 1: 550–57); also see, for example, Hillenkoetter, Memorandum for the President, March 16, 1948; CIA, Special Evaluation No. 27, March 16, 1948; Inglis, Memorandum of Information, March 16, 1948; CIA, "Possibility of Direct Soviet Military Action during 1948," April 2, 1948, HTL, HSTP, PSF, box 255; and CIA, "Review of the World Situation," April 8, 1948, *ibid.*, box 203.

⁷⁴ For NSC 7, see *FRUS, 1948*, 1: 545–50; for reactions and reservations of the State Department and the JCS, see *ibid.*, 557–64; and, for the support of Western Union, see *ibid.*, 3: 1–351. Also see, for example, materials in RG 218, Leahy Papers, boxes 5, 6; *ibid.*, ser. CCS 092 Western Europe (3-12-48). For military

The priority accorded to Western Europe did not mean that officials ignored the rest of Eurasia. Indeed, the sustained economic rejuvenation of Western Europe made access to Middle Eastern oil more important than ever. Marshall, Lovett, Forrestal, and other defense officials, including the joint chiefs, feared that American support of Israel might jeopardize relations with Arab nations and drive them into the hands of the Soviet Union. Although Truman accepted the partition of Palestine and recognized Israel, the United States maintained an embargo on arms shipments and sought to avoid too close an identification with the Zionist state lest the flow of oil to the West be jeopardized.⁷⁵ At the same time, the Truman administration moved swiftly in June 1948 to resuscitate the Japanese economy. Additional funds were requested from Congress to procure imports of raw materials for Japanese industry so that Japanese exports might also be increased. Shortly thereafter, Draper, Tracy S. Voorhees, and other army officials came to believe that a rehabilitated Japan would need the markets and raw materials of Southeast Asia. They undertook a comprehensive examination of the efficacy and utility of a Marshall Plan for Asia. Integrating Japan and Southeast Asia into a viable regional economy, invulnerable to communist subversion and firmly ensconced in the Western community, assumed growing significance, especially in view of the prospect of a communist triumph in China.⁷⁶ But communist victories in China did not dissuade policymakers from supporting, for strategic as well as domestic political considerations, the appropriation of hundreds of millions of dollars in additional aid to the Chinese nationalists in the spring of 1948. And the American commitment to preserve the integrity of South Korea actually increased, despite the planned withdrawal of occupation forces.⁷⁷

assistance, see *FRUS, 1948*, 1: 585–88; also see materials in RG 330, boxes 22 and 24, CD 6-2-46 and 6-2-49; RG 319, P&O, 092 (top secret); Lawrence S. Kaplan, *A Community of Interests: NATO and the Military Assistance Program, 1948–1951* (Washington, 1980); Condit, *History of the JCS*, 2: 409–36; and Chester J. Pach, “Arming the Free World: The Origins of the United States Military Assistance Program, 1945–1949” (Ph.D. dissertation, Northwestern University, 1981). For Germany, see especially *FRUS, 1948*, 2: 1–1340; and Smith, *Papers of General Lucius D. Clay*, 2: 527–969.

⁷⁵ See, for example, *FRUS, 1948*, 5: 545–54, 972–76, 1005–07, 1021–22, 1380–81; CNO to the Secretary of the Navy, January 24, 1948, NHC, CNO, double zero files, 1948, box 2; Leahy, Memorandum for the Secretary of Defense, October 10, 1947, RG 330, box 20, CD 6-1-8; Millis, *Forrestal Diaries*, 344–49, 356–65, 376–77; Bain, *March to Zion*, 137–213; and Miller, *Search for Security*, 173–203.

⁷⁶ For the rehabilitation of Japan, see Special Ad Hoc Committee [of SWNGC], Country Report on Japan, August 8, 1947, RG 333, box 109; Blum to Ohly, December 22, 1947, RG 330, box 9, CD 3-1-9; Blum to Forrestal, December 29, 1947, *ibid.*; Royall to Forrestal, April 28, 1948, *ibid.*; Royall, Memorandum for the Secretary of Defense, May 18, 1948, HTL, HS1P, PSF, box 182; and CIA, “Strategic Importance of Japan,” May 24, 1948, *ibid.*, box 255. Also see *FRUS, 1948*, 6: 654–56, 694–95, 712–17, 733–34, 750–51, 964–65. For Japan and Southeast Asia, see Ad Hoc Committee, “Study of a United States Aid Program for the Far East,” February 16, 1949, RG 319, P&O, 092 Pacific (top secret); and Schaller, “Securing the Great Crescent,” 392–414.

⁷⁷ In recent years scholars have shown that the limited aid to China was not simply a consequence of the influence of the China lobby and the administration's concern with the legislative fate of the European Recovery Program. Some policymakers (especially military officers) also were motivated by fear of the strategic and geopolitical consequences of a communist takeover in China, even though they fully recognized the ineptitude of the Chinese nationalists. See, for example, John H. Feaver, “The China Aid Bill of 1948: Limited Assistance as a Cold War Strategy,” *Diplomatic History*, 5 (1981): 107–20; Russell D. Buhite, “Major Interests: American Policy toward China, Taiwan, and Korea, 1945–50,” *Pacific Historical Review*, 47 (1978): 425–51; and Thomas G. Paterson, “If Europe, Why Not China? The Containment Doctrine, 1947–49,” *Prologue*, 13 (1981): 19–38. For a fine analysis of developments in both China and Korea, see Stueck, *Road to Confrontation*, 31–110; and, for aid to China, also see *FRUS, 1948*, 8: 1–269, 442–601.

The problem with all of these undertakings, however, was that they cost large sums, expanded the nation's formal and informal commitments, and necessitated larger military capabilities. Yet on March 24, 1948, just as NSC 7 was being finished, Truman's Council of Economic Advisors warned that accelerating expenditures might compel the president "to set aside free market practices—and substitute a rather comprehensive set of controls." Truman was appalled by this possibility and carefully limited the sums allocated for a build-up of American forces.⁷⁸ Key advisers, like Webb, Marshall, Lovett, and Clifford, supported this approach because they perceived too much fat in the military budget, expected the Soviets to rely on political tactics rather than military aggression, postulated latent U.S. military superiority over the Soviet Union, and assumed that the atomic bomb constituted a decisive, if perhaps short-term, trump card. For many American policy makers, moreover, the Iranian crisis of 1946, the Greek civil war, and the ongoing Berlin airlift seemed to demonstrate that Russia would back down when confronted with American determination, even if the United States did not have superior forces-in-being.⁷⁹

As secretary of defense, however, Forrestal was beleaguered by pressures emanating from the armed services for a build-up of American military forces and by his own apprehensions over prospective Soviet actions. He anguished over the excruciatingly difficult choices that had to be made between the imperatives of foreign economic aid, overseas military assistance, domestic rearmament, and fiscal orthodoxy. In May, June, and July 1948, he and his assistants carefully pondered intelligence reports on Soviet intentions and requested a special State Department study on how to plan American defense expenditures in view of prospective Soviet policies. He also studied carefully the conclusions of an exhaustive study of the navy's contribution to national security undertaken by the General Board of the navy under the direct supervision of Captain Arleigh Burke. Still not satisfied, Forrestal asked the president to permit the National Security Council to conduct another comprehensive examination of American policy objectives. Forrestal clearly hoped that this reassessment would show that a larger proportion of resources should be allocated to the military establishment.⁸⁰

⁷⁸ Edwin G. Nourse, Leon Keyserling, and Clark to Truman, March 24, 1948, HTL, HSTP, PSF, box 143; Truman to Nourse, March 25, 1948, *ibid.*; Statement by the President to the Secretary of Defense, the Secretaries of the Three Departments, and the Three Chiefs of Staff, May 13, 1948, *ibid.*, box 146; and Truman to Forrestal, July 13, 1948, RG 330, box 18, CD 5-1-20.

⁷⁹ For the views of Lovett and Webb, see Lovett diaries, December 16, 1947, January 15, 1948, April 21, 1948; for Clifford's view of the importance of the atomic bomb, see Clifford, Oral History, 88; and, for Marshall's reliance on the atomic bomb, see McNarney, Memorandum for the JCS, November 2, 1948, HTL, HSTP, PSF, box 114. Also see Policy Planning Staff [hereafter, PPS], "Factors Affecting the Nature of the U.S. Defense Arrangements in the Light of Soviet Policies," June 23, 1948, RG 330, box 4, CD 2-2-2; and Lovett to Forrestal, June 25, 1948, *ibid.* For the lessons derived from crisis decision making over Iran, Greece, and Turkey, see John R. Oneal, *Foreign Policy Making in Times of Crises* (Columbus, 1982).

⁸⁰ For the conflicting pressures on Forrestal and his own uncertainties, see, for example, Excerpt of Phone Conversation between Forrestal and C. E. Wilson, April 2, 1948, ML, JFP, box 48; Excerpt of Phone Conversation between Forrestal and Cannon, April 9, 1948, *ibid.*; and Forrestal to Ralph Bard, November 20, 1948, *ibid.*, box 78; for Forrestal's intense interest in the assessments of Soviet intentions, see Forrestal to Charles A. Buchanan [July 1948], RG 330, box 4, CD 2-2-2; and John McCone to Forrestal, July 7, 1948, *ibid.*, for Forrestal's request for a State Department study, see Lovett to Forrestal, June 25, 1948; for the naval study and Forrestal's interest therein, see General Board, "National Security and Navy," June 25, 1948; Arleigh

The Policy Planning Staff of the Department of State prepared the initial study that Forrestal requested and Truman authorized. Extensively redrafted it reappeared in November 1948 as NSC 20/4 and was adopted as the definitive statement of American foreign policy. Significantly, this paper reiterated the longstanding estimate that the Soviet Union was not likely to resort to war to achieve its objectives. But war could erupt as a result of "Soviet miscalculation of the determination of the United States to use all the means at its command to safeguard its security, through Soviet misinterpretation of our intentions, and through U.S. miscalculation of Soviet reactions to measures which we might take." Immediately following this appraisal of the prospects of war, the National Security Council restated its conception of American national security: "Soviet domination of the potential power of Eurasia, whether achieved by armed aggression or by political and subversive means, would be strategically and politically unacceptable to the United States."⁸¹

Yet NSC 20/4 did not call for a larger military budget. With no expectation that war was imminent, the report emphasized the importance of safeguarding the domestic economy and left unresolved the extent to which resources should be devoted to military preparations. NSC 20/4 also stressed "that Soviet political warfare might seriously weaken the relative position of the United States, enhance Soviet strength and either lead to our ultimate defeat short of war, or force us into war under dangerously unfavorable conditions." Accordingly, the National Security Council vaguely but stridently propounded the importance of reducing Soviet power and influence on the periphery of the Russian homeland and of strengthening the pro-American orientation of non-Soviet nations.⁸²

Language of this sort, which did not define clear priorities and which projected American interests almost everywhere on the globe, exasperated the joint chiefs and other military officers. They, too, believed that the United States should resist communist aggression everywhere, "an overall commitment which in itself is all-inclusive." But to undertake this goal in a responsible and effective fashion it was necessary "to bring our military strength to a level commensurate with the distinct possibility of global warfare." The Joint Chiefs of Staff still did not think the Soviets wanted war. But, given the long-term intentions attributed to the Soviet Union and given America's own aims, the chances for war, though still small, were growing.⁸³

Particularly worrisome were studies during 1948 suggesting that, should war

Burke, Oral History, NHC, 2: 30; and, for Forrestal's request and for a comprehensive study of American policy and Truman's responses, see Truman to Forrestal, July 13, 1948, RG 330, box 18, CD 5-1-20; Truman to Forrestal, July 15, 1948, HTL, HSTP, PSF, box 150; and *FRUS*, 1948, 1: 589-93.

⁸¹ NCS 20/1 and 20/4 may be found in Gaddis and Etzold, *Containment*, 173-211 (the quotations appear on page 208). Also see *FRUS*, 1948, 1: 589-93, 599-601, 609-11, 615-24, 662-69.

⁸² Gaddis and Etzold, *Containment*, 209-10.

⁸³ NSC 35, "Existing International Commitments Involving the Possible Use of Armed Forces," November 17, 1948, *FRUS*, 1948, 1: 656-62. For assessments of Soviet intentions and the prospects of war, see the citations in note 74, pages 374-75, above; also see CIA, "Possibility of Direct Soviet Military Action during 1948-49," September 16, 1948, HTL, HSTP, PSF, box 255; CIA, "Threats to the Security of the United States," September 28, 1948, *ibid.*, box 256; COMNAVFORGER, Intelligence Report, September 30, 1948, NHC, SPD, central files, 1948, box 245, FF61; JSPC, "Revised Brief of Short-Range Emergency Plan: Fleetwood," October 14, 1948, RG 218, ser. CCS 381 USSR (3-2-46), JSPC 877/23; and *FRUS*, 1948, 1: 648-50, and 5: 942-47.

occur, the United States would have difficulty implementing basic strategic undertakings. Although the armed services fought bitterly over the division of funds, they concurred fully on one subject—the \$15 billion ceiling on military spending set by Truman was inadequate. In November 1948, military planners argued that the \$14.4 billion budget would jeopardize American military operations by constricting the speed and magnitude of the strategic air offensive, curtailing conventional bombing operations against the Soviet Union, reducing America's ability to provide naval assistance to Mediterranean allies, undermining the nation's ability to control Middle East oil at the onset of a conflict, and weakening initial overall offensive capabilities. On November 9, the joint chiefs informed the secretary of defense that the existing budget for fiscal 1950 was "insufficient to implement national policy in any probable war situation that can be foreseen."⁸⁴

From the viewpoint of the national military establishment, the deficiency of forces-in-being was just one of several problems. Forrestal told Marshall that he was more concerned about the absence of sufficient strength to support international negotiations than he was about the availability of forces to combat overt acts of aggression, which were unlikely in any case. During 1948, the joint chiefs also grew increasingly agitated over the widening gap between American commitments and interests on the one hand and American military capabilities on the other. In November, the Joint Chiefs of Staff submitted to the National Security Council a comprehensive list of the formal and informal commitments that already had been incurred by the United States government. According to the joint chiefs, "current United States commitments involving the use or distinctly possible use of armed forces are very greatly in excess of our present ability to fulfill them either promptly or effectively." Limited capabilities meant that the use of American forces in any specific situation—for example, in Greece, Berlin, or Palestine—threatened to emasculate the nation's ability to respond elsewhere.⁸⁵

HAVING CONCEIVED OF AMERICAN NATIONAL SECURITY in terms of Western control and of American access to the resources of Eurasia outside the Soviet sphere, American defense officials now considered it imperative to develop American military capabilities to meet a host of contingencies that might emanate from

⁸⁴ JCS, Denfeld, Memorandum for the Secretary of Defense, November 8, 1948, RG 218, ser. CCS 370 (8-19-45), JCS 1800/14. For the impact of the \$14.4 billion budget on strategic plans, see JCS, "Allocation of Forces and Funds for the FY 1950 Budget," November 22, 1948, RG 330, box 16, "Draper: Budget File," JCS, 1800/18; for overall problems facing strategic planners, see the voluminous studies in RG 218, ser. CCS 381 USSR (3-2-46); and, for a few examples of the problems in implementing strategic plans, see JLPG, "Quick Feasibility Test of JSPG 496/4," March 19, 1948, *ibid.*, JLPG 84/5; JCS, "The Logistic Feasibility of Doublestar," August 12, 1948, *ibid.*, JCS 1844/15; JLPG, "Supply Priorities, for Fleetwood," October 15, 1948, *ibid.*, JLPG 84/31; and JLPC, "The Correction of Deficiencies Revealed by the Limited Feasibility test of ABC 101," December 23, 1948, *ibid.*, JLPC 416/36. In late 1948 and 1949 the navy challenged fundamental aspects of the strategic air offensive. See the studies in *ibid.*, ser. CCS 373 (10-23-48); also see Rosenberg, "Hydrogen Bomb Decision," 71–75. For service rivalries and the budgetary process, see, for example, Warner R. Schilling, "The Politics of National Defense: Fiscal 1950," in Schilling *et al.*, *Strategy, Politics, and Defense Budgets* (New York, 1962), 5–266.

⁸⁵ For the position of the JCS, see NSC 35, "Existing International Commitments," November 17, 1948, *FRUS*, 1948, 1: 656–62. For Forrestal's view, see *ibid.*, 644–46. For background, see William A. Knowlton,

further Soviet encroachments or from indigenous communist unrest. Such contingencies were sure to arise because American strategy depended so heavily on the rebuilding of Germany and Japan, Russia's traditional enemies, as well as on air power, atomic weapons, and bases on the Soviet periphery.⁸⁶ Such contingencies also were predictable because American strategy depended so heavily on the restoration of stability in Eurasia, a situation increasingly unlikely in an era of nationalist turmoil, social unrest, and rising economic expectations.⁸⁷ Although the desire of the national military establishment for large increments in defense expenditures did not prevail in the tight budgetary environment and presidential election year of 1948, the mode of thinking about national security that subsequently accelerated the arms race and precipitated military interventionism in Asia was already widespread among defense officials.

Indeed, the dynamics of the Cold War after 1948 are easier to comprehend when one grasps the breadth of the American conception of national security that had emerged between 1945 and 1948.⁸⁸ This conception included a strategic sphere of influence within the Western Hemisphere, domination of the Atlantic and Pacific oceans, an extensive system of outlying bases to enlarge the strategic frontier and project American power, an even more extensive system of transit rights to facilitate the conversion of commercial air bases to military use, access to the resources and markets of most of Eurasia, denial of those resources to a prospective enemy, and the maintenance of nuclear superiority. Not every one of these ingredients, it must be emphasized, was considered vital. Hence, American officials could acquiesce, however grudgingly, to a Soviet sphere in Eastern Europe and could avoid direct intervention in China. But cumulative challenges to these concepts of national security were certain to provoke a firm American response. This occurred initially in 1947–48 when decisions were made in favor of the Truman Doctrine, Marshall Plan, military assistance, Atlantic alliance, and German and Japanese rehabilitation. Soon thereafter, the “loss” of China, the Soviet detonation of an atomic bomb, and the North Korean attack on South Korea intensified the perception of threat to prevailing concepts of national security. The Truman administration responded with military assistance to southeast Asia, a decision to build the hydrogen bomb, direct military intervention in Korea, a commitment to station troops permanently in Europe, expansion of the American alliance system, and a massive rearmament program in the United States. Postulating a long-term Soviet intention to gain

Memorandum for the Chief of Staff, October 21, 1948, RG 319, P&O, 092 (top secret); for the reference to Greece, see JCS, “The Position of the United States with Respect to Greece,” April 13, 1948, RG 218, ser. CCS 092 Greece (12-30-47), JCS 1826/8.

⁸⁶ See, for example, the citations in notes 72–73, pages 373–74, above.

⁸⁷ See, for example, CIA, “The Break-Up of the Colonial Empires and Its Implications for U.S. Security,” September 3, 1948, HTL, HSTP, PSF, box 255; CIA, “Review of the World Situation,” September 16, 1948; and Inglis, Memorandum of Information, February 16, 1949, NHC, SPD, central files, 1949, box 249, A11.

⁸⁸ The view presented here of the expansive American conception of national security conflicts in part with the one presented by John L. Gaddis's *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy* (New York, 1981), 3–88. Gaddis's argument is thoughtful and insightful but relies too heavily on the recommendations of Kennan and his Policy Planning Staff. Indeed, the adoption of NSC 68 and the massive military build-up that accompanied the Korean War are much easier to understand if one grasps the expansive conception of national security that was pervasive in defense circles after World War II.

world domination, the American conception of national security, based on geopolitical and economic imperatives, could not allow for additional losses in Eurasia, could not risk a challenge to its nuclear supremacy, and could not permit any infringement on its ability to defend in depth or to project American force from areas in close proximity to the Soviet homeland.

To say this is neither to exculpate the Soviet government for its inhumane treatment of its own citizens nor to suggest that Soviet foreign policy was idle or benign. Indeed, Soviet behavior in Eastern Europe was often deplorable; the Soviets sought opportunities in the Dardanelles, northern Iran, and Manchuria; the Soviets hoped to orient Germany and Austria toward the East; and the Soviets sometimes endeavored to use communist parties to expand Soviet influence in areas beyond the periphery of Russian military power. But, then again, the Soviet Union had lost twenty million dead during the war, had experienced the destruction of seventeen hundred towns, thirty-one thousand factories, and one hundred thousand collective farms, and had witnessed the devastation of the rural economy with the Nazi slaughter of twenty million hogs and seventeen million head of cattle. What is remarkable is that after 1946 these monumental losses received so little attention when American defense analysts studied the motives and intentions of Soviet policy; indeed, defense officials did little to analyze the threat perceived by the Soviets. Yet these same officials had absolutely no doubt that the wartime experiences and sacrifices of the United States, though much less devastating than those of Soviet Russia, demonstrated the need for and entitled the United States to oversee the resuscitation of the industrial heartlands of Germany and Japan, establish a viable balance of power in Eurasia, and militarily dominate the Eurasian rimlands, thereby safeguarding American access to raw materials and control over all sea and air approaches to North America.⁸⁹

To suggest a double standard is important only insofar as it raises fundamental questions about the conceptualization and implementation of American national security policy. If Soviet policy was aggressive, bellicose, and ideological, perhaps America's reliance on overseas bases, air power, atomic weapons, military alliances, and the rehabilitation of Germany and Japan was the best course to follow, even if the effect may have been to exacerbate Soviet anxieties and suspicions. But even when one attributes the worst intentions to the Soviet Union, one might still ask whether American presuppositions and apprehensions about the benefits that would accrue to the Soviet Union as a result of communist (and even revolutionary nationalist) gains anywhere in Eurasia tended to simplify international realities, magnify the breadth of American interests, engender commitments beyond American capabilities, and dissipate the nation's strength and credibility. And,

⁸⁹ For Soviet losses, see Nicholas V. Riasanovsky, *A History of Russia* (3d edn., New York, 1977), 584–85. While Russian dead totaled almost twenty million and while approximately 25 percent of the reproducible wealth of the Soviet Union was destroyed, American battlefield casualties were three hundred thousand dead, the index of industrial production in the United States rose from 100 to 196, and the gross national product increased from \$91 billion to \$166 billion. See Gordon Wright, *The Ordeal of Total War* (New York, 1968), 264–65. For an analysis of Soviet threat perception in the aftermath of World War II, see Michael McGwire, "The Threat to Russia: An Estimate of Soviet Military Requirements" (manuscript in preparation at the Brookings Institution [title tentative]).

perhaps even more importantly, if Soviet foreign policies tended to be opportunist, reactive, nationalistic, and contradictory, as some recent writers have claimed and as some contemporary analysts suggested, then one might also wonder whether American capabilities, and dissipate the nation's strength and credibility. And, engender anxieties and to provoke countermeasures from a proud, suspicious, insecure, and cruel government that was at the same time legitimately apprehensive about the long-term implications arising from the rehabilitation of traditional enemies and the development of foreign bases on the periphery of the Soviet homeland. To raise such issues anew seems essential in the 1980s, when a correct understanding of an adversary's intentions, a shrewd grasp of an adversary's perceptions of vital interests, and a sound assessment of America's own national security imperatives seem to be indispensable prerequisites for the avoidance of nuclear war and the establishment of a safer climate for great power competition.

Comments:

MELVYN P. LEFFLER BUILDS HIS DISCUSSION of national security policy primarily upon archival sources emanating from the Pentagon, where a good deal of thinking about future wars was obviously going on in the late 1940s. These exercises required identifying certain vital interests, analyzing potential threats to those interests, and devising appropriate strategies to deal with those threats. That all of this was taking place is not particularly remarkable: modern military establishments regularly undertake such exercises in peacetime. What is remarkable is that Professor Leffler seems to have equated this planning with Washington's overall "conception of national security."

IF ONE WERE GOING TO SET ABOUT INVESTIGATING that subject, one would of course be interested in how war planners identified interests and assessed threats, but to stop at that point would be to risk a certain myopia. Would one not also want to consider what other agencies within the government were assigned responsibilities in this area, and what their relative influence was on actual policy? Would one not also want to know what resources were available with which to defend vital interests, and on what basis were they allocated? Would one not also want to have some sense of what triggered concern about national security in the first place, and the extent to which other nations shared that concern? And would one not want to take into account the nature of the international system itself, and the extent to which it shaped the evolution of thinking about "national security"? On all of these points, Professor Leffler's analysis leaves me less than satisfied.

First, with regard to relative influence within the government, it may well be that my own recent treatment of this subject in *Strategies of Containment* gives too much emphasis to the role of George Kennan and the Policy Planning Staff. Well-meaning friends, colleagues, and students remonstrate with me regularly on this point, though to little avail. But Professor Leffler's attempt to compensate for my shortcomings by excluding not only the Policy Planning Staff but the entire State Department almost completely from his discussion of national security policy strikes me as going a bit far. It is as if one had set out to write a history of the Nixon administration's foreign policy from the viewpoint of William P. Rogers, with only an occasional mention of Henry Kissinger.

Professor Leffler would be hard-pressed, I think, to sustain the thesis that the Pentagon's recommendations on national security policy carried more weight in the

Truman White House than did those of Secretary of State George Marshall and his subordinates in the department. And yet, that is precisely the suggestion his essay conveys, whether intentionally or not. We are left, as a result, with some curious impressions. It is interesting to learn, for example, that Moscow's diplomatic demands did little to influence Washington's assessment of the Soviet threat. Students of the de-industrialization policies of Generals Clay and MacArthur (and of Kennan's memoirs) will be intrigued to find out that the Pentagon was in the vanguard of those seeking to rebuild German and Japanese industrial strength as a bulwark against the Russians. But the strangest suggestion of all is that Washington's key decision to rely on the economic rather than the military instruments of containment in the late 1940s originated in the Pentagon and not the State Department; that the real father of the Marshall Plan was not Kennan, or Dean Acheson, or Will Clayton, or even Marshall himself, but rather James Forrestal.

What Professor Leffler has confused here is the distinction between acquiescence and initiation. Of course the Defense Department and the armed services went along with the decision to emphasize an economic over a military response: given the tradition of civilian control in foreign policy, given Marshall's own immense authority and prestige, given the military's inability to suggest alternative courses of action that fit the president's budgetary restrictions, the services had no alternative other than to squabble among themselves—as they vociferously did—over how their own remaining portion of government revenues would be divided. That fact hardly justified the implication, strongly set forth in this essay, that the Pentagon was in fact determining this critical aspect of national security policy.

This brings up a second and related issue. Professor Leffler makes occasional references to the Pentagon's budgetary problems, but he gives no sense of how severe these were in the late 1940s, or to what extent they constrained strategic planning. Any organization whose budget drops from \$81 billion to \$13 billion in the course of two years—as the total military budget did between 1945 and 1947—must undertake a certain amount of retrenchment. Moreover, Leffler fails to mention at all the problems of demobilization, which had slashed available manpower during that same period from 12.1 million to 1.6 million. The essence of strategy—and, hence, of national security policy—is the matching up of interests with capabilities, the squaring of one's "wish list" with one's checkbook. Leffler has given us a good discussion of what the planners wanted, but he has almost entirely neglected the question of what they thought they could afford. Nor has he considered what these budget figures tell us about the Pentagon's overall influence within the government at that time.

A word about the atomic bomb is in order here as well, since Professor Leffler sees it—as have many others—as a substitute for the obvious shortcomings of U.S. conventional forces during this period. Leffler cites the recent work of David Rosenberg, Gregg Herken, and Harry Borowski, which has added greatly to our knowledge of the nature and size of the American atomic arsenal in the years before the Russians got the bomb. What he has not told us is that all three of those authorities go on to discuss at some length the doubts among American military

leaders themselves about whether atomic bombing alone would suffice to defeat the Russians should war come, given the high number of targets that would have had to be hit and given the relatively small number of bombs—and properly equipped bombers—available with which to hit them. The weapon may have been regarded as an awesome deterrent, but it was certainly not seen as an absolute counterweight to Soviet conventional force superiority in Europe.

A third difficulty with Professor Leffler's analysis has to do with this very question of how the Soviet threat was perceived in the first place. Leffler fails to distinguish clearly enough, in my view, between the Russian military threat to North America, which he correctly says no one took very seriously at that time, and the possibility that the Red Army might overrun Western Europe, which was something else again. Although intelligence reports discounted the probability of a deliberate attack in Europe, they by no means disregarded the possibility of hostilities beginning as the result of accident or misperception. And, given Soviet conventional force superiority at that time, the Russians would have had the capability to overrun most of Western Europe in a matter of weeks, a fact all American war plans during this period took for granted. If, as Leffler rightly argues, American planners regarded it as a vital interest to deny the combined resources of Eurasia to potentially hostile powers, then this Soviet capability had to be regarded as a threat of the first order, in view of the weaknesses of American and Western European conventional forces and the questionable utility of the atomic bomb. But Leffler gives scant attention to these deficiencies.

Significantly, Washington planners were not alone in perceiving this Soviet threat. The West Europeans took it even more seriously than the Americans did and, as a consequence, set out in 1948 to persuade the United States that its own security required the extension of credible military guarantees to cover them as well. I find it very surprising that Professor Leffler has not seen fit to make any reference at all to the role the West Europeans played in modifying American policy in this regard and only the most fleeting references to the negotiations that eventually produced the North Atlantic Treaty—a document of some importance, one might think, in the evolution of the American conception of national security.

Finally, Professor Leffler fails to set his analysis within the framework of what we know to be the nature of the international system itself. He appears to feel that he is telling us something new when he reveals that the Americans were not all that idealistic after all, that they were in fact out to promote their own interests in the world, and that, when confronted with opportunities, they took advantage of them. This seems to me analogous to not discovering sex until the age of, say, forty-two. I think we can take it for granted that Americans were not exempt from the temptations of power that have afflicted all great nations at one time or another; surely Stalin and his associates, with their own exceedingly cynical view of human nature, can hardly have expected us to behave in any other way.

The interesting question for students of the Cold War is not whether the two major antagonists sought and obtained great power but how they went about seeking it and what they did with it once they had it. How was it, for example, that the postwar expansion of American influence in the world set off so few fears

among the wide variety of people and nations affected by it, while the expansion of Soviet influence, despite the fact that it took place on a considerably smaller geographical scale, set off so many? Answering questions like this will require a greater familiarity with foreign sources and a greater facility in the techniques of comparative history than are commonly found among diplomatic historians in this country. But it is time we directed our energies to this task, and away from attacking the now thoroughly shredded straw man of American naiveté and idealism.

ONE OF GENERAL MARSHALL'S RECURRING FRUSTRATIONS during World War II involved what he liked to call "theater-itis"—the tendency of individual commanders to become so caught up in their own campaigns that they lost sight of how those fit into the larger strategy of global war. Despite the impressive amount of research that has gone into it, Professor Leffler's essay, I fear, shows the effects of a related syndrome, "archive-itis"—the tendency of historians to become so immersed in particular archives that they lose sight of that larger context into which all archival revelations must eventually be set. We can all applaud Leffler's energy in mining the documents he cites. We can anticipate that this will be the first of many contributions to Cold War studies from a scholar who has distinguished himself admirably in other areas of American diplomatic history. But may we not also express the hope that next time Professor Leffler will turn his attention to the conceptual forest as well as to the constituent trees?

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MELVYN LEFFLER'S MAIN ARGUMENT appears to be that the American conception of national security during the early Cold War was a consequence not so much of Soviet actions as of America's perceived vulnerabilities and of its resulting strategic and economic imperatives. Central to his argument, once its elements are pieced together and the rhetorical questions and qualifications in the conclusion carefully examined, is a belief that implementation of this conception was unnecessarily provocative and, hence, primarily responsible for many of the Cold War's most enduring characteristics. While thought-provoking, Leffler's argument has problems. My quarrel with his analysis focuses on four of them: (1) how valid is the conception that he delineates; (2) what factors were most important in its development; (3) what caused the action-reaction syndrome of which that conception was a part; and (4) was the conception itself primarily at fault?

THE "AMERICAN CONCEPTION OF NATIONAL SECURITY" is a troubling concept. Was it truly an "American" conception, or was it the conception of defense officials who looked at U.S.-Soviet relations from a military perspective (as was their responsibility) and attempted to meet the growing military requirements of evolving U.S. policies? The distinction between "American" and "defense" is important. Defense officials were constrained in implementing their conceptions by the Department of State, not to mention the president and Congress; as a result, they were only partially successful—and then only *after* Soviet actions (in, for example, Iran and Turkey) made such implementation possible.

Even if we assume the partial validity of the "American" conception, the problem of its origins is subject to question. In my judgment, strategic and economic imperatives *help* explain the development of national security conceptions in the early Cold War. Far more can be explained, however, including the *urgency* of strategic and economic imperatives, by Soviet actions in the Near East in the period 1944–46 and by the legitimate fears that such actions engendered. These fears emerged not only among the governments that the Soviets intended to intimidate but also within the Department of State, which was primarily responsible for U.S. policy during the early Cold War. As a consequence, Soviet actions (not just *perceptions* of Soviet actions) must be given far greater weight than Professor Leffler gives them; the international political contexts within which conceptions of national security developed, moreover, must be explored in much greater depth if we are to understand why those conceptions developed in the manner that they did.

The question of how important implementation of the American conception of national security was to the beginnings of the Cold War is one that Professor Leffler raises but does not satisfactorily answer. The suggestion in his conclusion that Soviet policies may have been formulated in reaction to U.S. policies and conditioned by legitimate apprehensions about U.S. intentions has the ring of reasonableness and balance. It appears consistent with a litany of American policies that historians have cited to explain certain aspects of Soviet behavior toward the United States in the early Cold War.¹ But his argument needs careful scrutiny—and not merely from the standpoint of chronology.² To be convincing, he needs to provide us with a better understanding of the Soviet conception of national security. As stated in his essay, however, he does not pretend to discern the objectives and motivations of the Soviet Union.

Vojtech Mastny, a historian who has attempted precisely this endeavor, has

¹ These policies include the delay in the opening of a second front, unilateral decision making (such as the establishment of a separate U.S. and British Control Commission in Italy), the abrupt cancellation of lend-lease, and the failure to accept international control of atomic energy.

² In a paper he delivered in 1983, Leffler argued that developments based on strategic considerations, which exerted an important influence on the course of American foreign policy toward Turkey from the time of the Truman Doctrine, may well have accentuated Soviet fears, magnified their sense of weakness, and intensified their suspicions of American intentions. The problem with this argument is that it discounts the importance of Soviet policies toward Iran and Turkey in 1944–46, casts those policies in the context of a legitimate reaction to unspecified U.S. policies, and ignores their *major* influence on decision makers responsible for determining U.S. strategic imperatives. As a result, subsequent Soviet behavior is characterized as a response to action *initiated* by the United States, Turkey, and N.A.T.O., 1945–1952," paper presented at the Seventy-Sixth Annual Meeting of the Organization of American Historians, held in Cincinnati, April 6–9, 1983.

convincingly argued, I believe, that Stalin's objectives were not very different from the traditional goals of Russian imperialism; the primary source of conflict between East and West, he asserted, was Russia's striving for power and influence far in excess of its reasonable security requirements. Mastny suggested that Stalin, premising success on the ability to rule his empire without arousing alarm about his intentions, could have taken a more enlightened and less exaggerated view of what security meant only if he had not been Stalin.³ Professor Leffler, apparently, rejects Mastny's interpretation. Here and in earlier writings on the subject, Leffler's treatment of Soviet policies in the Near East raises a problem that is symptomatic of his analysis. He plays down the significance of serious Soviet pressures on Iran and Turkey, placing such actions, whose importance is central to the debate, in the context of contradictory evidence of Soviet intentions. The disagreement between us on this matter is profound and will not be resolved here,⁴ but it may be useful to speculate how Soviet leaders themselves viewed Stalin's behavior toward Turkey, which, along with Iran, was a focal point of U.S.-Soviet confrontation during the early Cold War.

In spite of Soviet attempts to distort the record of their relations with Turkey,⁵ former Soviet Premier Nikita Khrushchev's memoirs, corroborated by voice print, give some indication of what may have motivated Stalin's policies. According to Khrushchev, Lavrenti Beria, head of Stalin's huge police network, and, like Stalin, a Georgian, teased and goaded Stalin into demanding the return of territories that had once (from 1878 to 1921) been part of Georgia. Beria argued that Turkey was weakened by World War II and would be unable to resist such demands. As Khrushchev himself acknowledged, Beria and Stalin "succeeded only in frightening the Turks right into the open arms of the Americans. *Because* of Stalin's note to the Turkish government, the Americans were able to penetrate Turkey and set up bases right next to our borders."⁶ In a note of May 30, 1953, Stalin's successors, less than three months after his demise, informed the Turks that the governments of Armenia and Georgia had renounced their territorial claims against Turkey; they also stated that, after reconsidering the question of the Straits, they believed Soviet security could be assured by conditions acceptable to Turkey—an unusual public retraction and tacit admission (repeated again in 1965 by Soviet President Podgorny) of Stalin's irresponsible international ambitions.⁷ How we should regard such fragments of evidence, admittedly, will always be problematic. If Professor Leffler intends to argue that the development of U.S. capabilities affected Soviet behavior

³ Mastny, *Russia's Road to the Cold War: Diplomacy, Warfare, and the Politics of Communism, 1941–1945* (New York, 1979), 35, 283, 292, 306.

⁴ For our respective views, see Bruce R. Kuniholm, *The Origins of the Cold War in the Near East: Great Power Conflict and Diplomacy in Iran, Turkey, and Greece* (Princeton, 1980); and Melvyn P. Leffler, Review of my *The Origins of the Cold War*, in "From Cold War to Cold War in the Near East," *Reviews in American History*, 9 (1981): 124–30, esp. 128, and "Strategy, Diplomacy, and the Cold War."

⁵ See Kuniholm, *Origins of the Cold War in the Near East*, 220 n. 19, 262 n. 135, 264 n. 143.

⁶ Strobe Talbot, trans. and ed., *Khrushchev Remembers: The Last Testament* (Boston, 1974), 295–96 (italics added). Also see Kuniholm, *Origins of the Cold War in the Near East*, 358–59.

⁷ Firenc Váli, *Bridge across the Bosphorus: The Foreign Policy of Turkey* (Baltimore, 1971), 174–75. Also see the statement by Podgorny in Izmir on January 11, 1965. Foreign Broadcast Information Service, *Daily-Report*, January 6, 1965, pp. M1–3, January 7, 1965, pp. M1–5, and January 11, 1965, pp. M1–4.

to the extent he suggests, however, he cannot avoid the tough questions—how it did so, when it did so, and what evidence supports the assumption that it did so—and then ask us, at the end of his argument, to consider the very question that his analysis begs.

The real issues between us appear to be the norms that one should apply to the difficult judgments that historians make: how does one define “legitimate” security concerns, and to what extent can dominant perceptions and policies (whether Soviet or American) be seen as “legitimate” in the period under consideration? Given “legitimate” Soviet security concerns, the question is whether Soviet behavior in 1944–46 was appropriate to those concerns. Professor Leffler appears to think that it was. I think that it was not. Our answers to this question qualify our different definitions of “legitimate” in the next question: given “legitimate” U.S. security concerns, was U.S. policy in 1946–47 appropriate? Leffler appears to think that it was not. I think that it was. It is one thing not to expect an attack against North America; it is quite another thing to be concerned that intimidation, over time, could make possible the expansion of excessive Soviet influence. In the case of the Near East, the problem for U.S. officials was hardly ever whether or not a Soviet attack was imminent, but whether Soviet intimidation, unopposed by the United States, would force the countries on its southern flank to accommodate Soviet interests; the extent to which those countries might have to do so; and whether the United States, the only power that could oppose Soviet pressures, should reject requests for assistance by the governments in question and acquiesce in such developments. From the point of view of U.S. officials, the issue, ultimately, was how to respond to repeated Soviet pressures—before rather than after they became a problem—in a manner consistent with reasonable interpretations of what the Soviets were doing, sufficient to deter but not precipitate Soviet initiatives, and *comprehensible enough to ensure domestic support from a public confused by wartime rhetoric*. Recent history indicated to U.S. and other, foreign officials that the Soviets would not act in a manner appropriate to reasonable standards of international behavior and that something had to be done about it. The driving fact in the region was inauspicious Soviet behavior that provided a context within which developing (but not yet formally accepted) strategic conceptions gained currency. Not to have anticipated such Soviet behavior, particularly after the long and drawn-out crisis in Iran in 1945–46, and in view of previous pressures on Turkey, would have constituted a dereliction of responsibility.

If, as Professor Leffler asserts, the Soviets had legitimate security interests in Eurasia, their actions in Iran, following as they did their expansion into Eastern Europe and the Far East (accompanied by extremely harsh methods of control), superseded the bounds of what a majority of the international community was prepared to accept. What would be done to oppose similar Soviet actions elsewhere along the Middle East’s “Northern Tier” or Europe was not clear. The mood of the American public was uncertain. That is why, in spite of its shortcomings, something like the Truman Doctrine may have been necessary. Whether Soviet pressures on Iran and Turkey can be justified by the USSR’s enormous losses during World War II, or by the assertion, made by some, that the United Nations advanced

American interests and power at Soviet expense, depends on one's point of view.⁸ What is striking is the extent to which, in the eyes of those whose territorial integrity was in question, U.S. interpretations of events were seen as more accurate and U.S. concerns were seen as more legitimate than those of the Soviet Union. U.S. involvement in the affairs of Iran and Turkey, finally, was encouraged by those countries, because their governments wanted the United States to serve as a counterweight to the USSR, whose influence was resented and feared.

The problem posed by Iranian, Turkish, and finally Greek requests for support and assistance raises the fundamental question of what strategic imperatives were reasonable for the United States in view of Britain's traditional role as a balance to Soviet power in the region, the decline of the British Empire after World War II, and Soviet policies toward Iran and Turkey in the early Cold War. Was the U.S. conception of national security, as gradually implemented in the Near East, excessive and hence "illegitimate"? Should the Near East have been incorporated within a Soviet sphere of influence? Did history sanction such an arrangement? Should the United States have agreed to it, and, if so, what would the consequences have been? Would it have been right for the United States to have done less than it did? The answers to all of these questions, I would submit, is "No." If the USSR did not have the wherewithal to stop the emergence of an offensive threat on its southern border in 1947–48, as Professor Leffler has noted elsewhere,⁹ neither did the Turks and Iranians, short of outside assistance, have what was necessary to deter (they could not have stopped) the Russians in 1944–47.¹⁰ What was known of Soviet pronouncements on the Balkans, and the inferences that could be drawn from the Soviet Union's performance in Iran and Turkey, moreover, strongly suggested that the Soviet Union would take advantage of any situation that proved favorable to its interests in the region. If Mastny was correct in asserting that, after Potsdam, Stalin's policy was irrevocable, that Stalin tightened his grip wherever it reached, and that he tested soft spots in the hope of grasping still more, the Truman Administration's policies along the Northern Tier, while overreactive and provocative in Leffler's view, would appear to have been consistent with the kinds of policies that former Soviet Foreign Minister Maxim Litvinov believed the United States should have pursued in Eastern Europe and the Balkans during World War II.¹¹

⁸ For elaboration of some of these issues, see Kuniholm, *Origins of the Cold War in the Near East*, 160–63, 205–07, 428–32.

⁹ Leffler, "Strategy, Diplomacy, and the Cold War," 18.

¹⁰ Soviet inability to carry a war to the United States, it should be noted, was not very reassuring. Even if defense officials were confident the Soviets did not feel strong, the Soviets did have overwhelming conventional capabilities. U.S. war plans assumed a rapid takeover of Eurasia, as Leffler notes, and countries like Czechoslovakia could be taken over without any alteration in Soviet tactics.

¹¹ Mastny, *Russia's Road to the Cold War*, 283, 305. In June 1945, Litvinov told the journalist Edgar Snow that the United States should have begun opposing the Soviets in the Balkans and Eastern Europe as far back as 1942; a year later he told CBS correspondent Richard Hottelet that differences between East and West had gone too far to be reconciled and that the root cause was the prevailing ideological conception in the Soviet Union of the inevitability of conflict between the communist and capitalist worlds. Asked if Soviet suspicions of the West would be mitigated if all Russian demands were granted, Litvinov saw little hope and volunteered that there was nothing one could do inside a totalitarian state to change it. For records of the two conversations, see Edgar Snow, *Journey to The Beginning* (New York, 1958), 357; James F. Byrnes Papers, Clemson University Library, Clemson, SC, folder 638; and *Foreign Relations of the United States, 1946, 6* (Washington, 1969): 763–65.

WHAT THIS INTERPRETATION SUGGESTS is that, contrary to Professor Leffler's view, a fuller understanding of Soviet perceptions and intentions during the early Cold War might well have resulted in more or less the same policies that the United States pursued. Leffler may be reluctant to make judgments about what Soviet perceptions and intentions were, but American policy makers in the 1940s were unable to afford such a luxury. Their conceptions of national security interests required difficult judgments about the Soviets that had to be made on the basis of available evidence, without the knowledge of how history would turn out. Although their analyses clearly could have been more subtle, and their policies better managed, the conceptions they began to develop were not inconsistent with emerging postwar realities and do not appear to have been unreasonable in view of Soviet behavior. While I share Professor Leffler's concerns about U.S. policies toward the Soviet Union in the 1980s and believe that the questions he raises are extremely important, I do not believe that such concerns, however warranted in theory, actually legitimate Soviet policies in the 1940s; as a result, I am unconvinced by his explanation of the beginnings of the Cold War and do not share his judgment that "the American conception of national security," in and of itself, was as important as his title suggests it was in explaining how the Cold War began.

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Reply:

DURING THE LAST DECADE the stimulating and informative works of John Lewis Gaddis and Bruce Kuniholm have attracted much attention and have revived traditional views of the origins of the Cold War. Accordingly, I am pleased to have the opportunity to respond to their comments. Although they raise some important issues, I find their critiques unconvincing.

Both Professors Gaddis and Kuniholm suggest that my account is misleading because I focus on Pentagon planners and ignore the views of influential civilian policy makers. In fact, however, the highest civilian officials in the State, War, and Navy departments concurred in and supported the conception of national security I describe in my essay. My narrative and documentation demonstrate that Byrnes, Marshall, Lovett, Forrestal, and Patterson repeatedly endorsed the need for the development of overseas bases, the negotiation of air-transit rights, the control of polar air routes, the consolidation of a strategic sphere of influence in Latin America, and the preservation of a favorable balance of power in Eurasia. The documents from which I derived this conception of national security include not only the “exercises” of anonymous bureaucrats, as Gaddis suggests, but also the policy papers approved by the secretaries of the State, War, and Navy departments, reports written by Truman’s closest civilian aides like Clark Clifford and George Elsey, and the studies of the National Security Council (for example, NSC 20/4), which were approved by the president and became national policy.

The view of Professors Gaddis and Kuniholm that State Department officials possessed a different perspective of national security is unsubstantiated and misleading. Between 1946 and 1948, the men at Foggy Bottom certainly did not think it wise to spend scarce funds on military capabilities. But to infer from this that the State Department (or the president) had a narrower conception of national security is erroneous. Such a suggestion from Kuniholm is especially ironic because a major part of his *Origins of the Cold War in the Near East* (1980) demonstrates how Foreign Service officers assumed the initiative in projecting American interests into Iran, Greece, and Turkey. Inasmuch as Gaddis is so concerned with NATO, he, too, is aware that State Department officials like John Hickerson and Theodore Achilles were among the most prominent of the policy makers who saw considerable utility in assuming military commitments in Europe, however ill-defined they might be. Subsequently, other State Department officials, like George McGhee, were among the strongest supporters of the policy expanding American commitments into the Near East by including Greece and Turkey in NATO. Likewise, the

most ardent champions of containing Soviet influence in Korea resided not in the Pentagon but at Foggy Bottom. Although Acheson, Kennan, and some of their colleagues did caution against over-commitments in China, the reason did not lie in any divergent conception of national security. Rather, they did not think that the “loss” of China would affect the overall balance of power in Eurasia. In their view, China was too unstable, too poor, and too weak to become a major asset to Soviet Russia, even if the Kremlin could control developments in Peking or the Chinese countryside—which many policy makers doubted.¹

Although generalizations are always fraught with difficulties, I do not think it accurate to imply that State Department officials had a narrower conception of American interests than defense officials did. In fact, they probably were even more inclined to expand American commitments, except in China. Defense officials, however, did worry more about the gap between commitments and capabilities. Hence, they were the first to call for increased military expenditures. But too much attention should not be focused on this point, because Professor Gaddis himself has noted that by late 1949 or early 1950 State Department and other civilian officials may have surpassed even the military planners in their advocacy of larger military appropriations.²

In this context, Professor Gaddis’s repeated criticism of my treatment of Secretary of State George Marshall puzzles me. Gaddis does not contend, for example, that Marshall had a view of national security different from the one I depict. Rather, Gaddis claims that I do not give Marshall’s State Department enough credit for the very definition of security I describe. But the significance of this point eludes me, especially since Marshall embodied the kind of defense official I refer to in my essay who, despite his military background, remained extremely sensitive to the primacy of socioeconomic considerations and political values. Nor should it be forgotten that General Marshall, upon becoming secretary of state, chose Robert Lovett, a former assistant secretary of war, as his under-secretary. Lovett, in turn, persuaded Colonel Bonesteel to leave the War Department and become a principal assistant for European affairs. Meanwhile, Marshall and Lovett maintained General Hilldring as the assistant secretary of state for occupation affairs. These men and others (like John McCloy) could and did move from the armed services and defense departments to the highest rungs of the State Department (and other agencies) precisely because there was a consensus on fundamental objectives.

Part of this consensus involved a shared apprehension of the grave consequences of economic dislocation, social unrest, and political instability. Although I never claim that Forrestal was the originator of the Marshall Plan and would not contest the State Department’s pride of authorship, the significant point is that the highest officials in the defense agencies in late 1946 and early 1947 shared the view that economic rehabilitation should take priority over military assistance and domestic

¹ Kuniholm, *Origins of the Cold War in the Near East*; for NATO, see, for example, *FRUS, 1948*, 3: 1–351; for McGhee, see *ibid.*, 1951, 5: 1–11, 21–42, 1113–20; for the most recent views on Korea, see Bruce Cumings, ed., *Child of Conflict: The Korean-American Relationship, 1943–1953* (Seattle, 1983), xii, 3–38, 169–93; for China, see, for example, *FRUS, 1948*, 8: 146–55, 208–11; Borg and Heinrichs, *Uncertain Years*, 13–52.

² Gaddis, *Strategies of Containment*, 92–95.

rearmament. Defense officials recognized the need to resuscitate the German and Japanese economies, not initially as a bulwark against the Russians, as Gaddis misleadingly represents my argument, but as a response to a multitude of local and regional economic and social problems. Those who have read the works of John Gimbel, John H. Backer, J. W. Dower, Howard Schonberger, and Takeshi Igarashi on the occupations of Germany and Japan should have no difficulty accepting this interpretation.³

As for Professor Gaddis's stress on the impact of budgetary constraints and postwar demobilization, I agree that the desire to balance the budget and bring the boys home influenced the tactical implementation of national security policy. But these considerations did not have an important bearing on the initial postwar conceptualization of national security objectives (which is the subject of my essay). When faced with the gap between goals and capabilities, the thrust of the Truman administration's policy was almost always to expand capabilities (first in the form of economic aid, then military assistance, and, after 1950, rearmament) rather than to narrow goals. That the military budget did not expand during 1946, 1947, and 1948 does not refute my argument, as Professor Gaddis thinks, because top policy makers were always cognizant that arms expenditures constituted only one means of achieving national security goals. As long as the major threat remained socioeconomic unrest rather than prospective Soviet military aggression, top defense officials were willing to assess the full spectrum of American capabilities to determine which should receive priority.

In expanding capabilities policy makers had to make tough decisions. They were compelled to rank their goals and to fight with one another over tactics. They had to determine the relative importance of military assistance to Latin America versus economic aid to Europe; they had to choose between balanced budgets and expenditures on overseas bases, new aircraft carriers, long-range bombers, and relief assistance to Japan and Germany; they had to accommodate the security requirements of France and deter a communist triumph in that nation while satisfying the needs of German and Western European economic rehabilitation. These decisions engendered conflicts within and among the departments dealing with national security; many of these decisions have been the subject of fine studies by scholars influenced by theories of bureaucratic politics. But differences over tactics and priorities should not obscure the shared objectives held by top government officials. Even scholars who have stressed the centrality of bureaucratic politics, like Graham Allison and Morton Halperin, have acknowledged that policy makers usually do share a set of fundamental values and agree on basic facts.⁴ Too frequently, however, these fundamentals are ignored and their implications left unexplored.

³ Gimbel, *Occupation of Germany*; Gimbel, *The Origins of the Marshall Plan* (Stanford, 1976); John H. Backer, *Priming the German Economy: American Occupational Policies, 1945–1948* (Durham, N.C., 1971), and *Decision to Divide Germany*; Dower, *Empire and Aftermath*; Schonberger, "Zaibatsu Dissolution and the American Restoration of Japan," *Bulletin of Concerned Asian Scholars*, 5 (1973): 16–31; and Igarashi, "MacArthur's Proposal for an Early Peace with Japan and the Redirection of Occupation Policy toward Japan," *Japanese Journal of American Studies*, 1 (1981): 55–86.

⁴ Graham T. Allison and Morton H. Halperin, "Bureaucratic Politics: A Paradigm and Some Policy Implications," in Raymond Tanter and Richard H. Ullman, eds., *Theory and Policy in International Relations* (Princeton, 1972), 56.

The critical question raised in Professor Kuniholm's comments is whether these fundamental objectives of American national security policy were or were not a result of Soviet actions. Kuniholm's major criticism of my analysis rests on his contention that the American conception of security was the result of Soviet intimidation. But Kuniholm appears unaware of the careful assessment of American interests in the Near East during July 1945, *before* the dispute over northern Iran, before the Soviet request for bases in the Dodecanese or Tripolitania, and before all those reports on troop movements that Professor Kuniholm cited in his book. During this assessment, on the eve of the Potsdam meeting, two senior American officers on the Joint Strategic Survey Committee argued in favor of acceding to Soviet ambitions in the Turkish straits. Their viewpoint was powerfully opposed by the army's Strategy and Policy Group, by the navy, and by the State Department. The JCS discussed the issue; McCloy reviewed it with his military experts; and the secretary of war submitted his views to the secretary of state. The result was a decision to oppose Soviet bases in the Dardanelles, Soviet territorial demands, and any aggrandizement of Soviet influence in the area. "The United States," it was argued, "must seek to prevent the growth of any single power or coalition to a position of such strength as to constitute a threat to the Western Hemisphere. . . . To this end, our long range policies in Europe and Asia must be in opposition to Russian expansion into Western Europe and Central and Southeast Asia." Noting that Soviet inroads into Asia Minor might facilitate Soviet control of the Eastern Mediterranean, the Aegean, and the Persian Gulf, American officials insisted that this would threaten the lifeline of the British empire and jeopardize interests of "central strategic importance" to the United States.⁵

Not only did the American conception of security in the Near East region precede the Iranian and Turkish crises of 1946, but it is questionable whether Soviet actions toward Turkey constituted a "war of nerves" or "intimidation," as Professor Kuniholm insists they did. It is important to note that Kuniholm no longer focuses on reports of Soviet troop movements. Acknowledging that intelligence information during late 1945 and 1946 demonstrated little likelihood of a Soviet attack, Kuniholm now places emphasis on other signs of Soviet intimidation.⁶ But his evidence remains equally dubious. For example, alluding to the Soviet note to the Turkish government in August 1946, Kuniholm relies on Khrushchev's account of Beria goading Stalin into making territorial demands. But, in fact, this Soviet note made no territorial demands whatsoever. During the preceding months Soviet diplomats had intimated that they were flexible on territorial revision and that it was not a high priority issue. After the middle of 1946, moreover, the Soviets apparently ceased raising the territorial claim in diplomatic conversations with the Turks, or at least in those talks reported to American diplomats.⁷

⁵ The quotations appear in Strategy and Policy Group, OPD, "U.S. Position Relative to Soviet Intentions in Turkey and the Near East," July 6, 1945, RG 165, ser. ABC 092 USSR (11-15-44); JCS, "United States Policy concerning the Dardanelles and Kiel Canal," July 12, 1945; JCS, Minutes of the 169th Meeting, July 17, 1945, RG 218, ser. CCS 092 (7-10-45); JCS to Secretary of State, July 30, 1945, *ibid.*; Embick to Handy, July 4, 1945, RG 165, OPD, 336 (top secret); Lincoln to McCloy, July 6, 1945, *ibid.*, ser. ABC 093 Kiel (7-6-45); Secretary of War to Secretary of State [July 1945], *ibid.*; and Thomas D. Roberts to Lincoln, July 16, 1945, *ibid.*

⁶ For the intelligence reports, see Leffler, "Strategy, Diplomacy, and the Cold War," 4-5.

⁷ For the Soviet note, see *FRUS*, 1945, 7: 827-29. For earlier discussions in which Soviet diplomats played down the territorial issue, see *ibid.*, 812-13, 816, 826. I have been unable to find in the State Department

Of course, the Soviets cared much more about bases in the straits than they did about territorial revision. In his book Professor Kuniholm repeatedly referred to Soviet efforts to discuss this matter as constituting a “war of nerves” or “intimidation.” For example, alluding to a discussion between Stalin and Ambassador Smith in April 1946, he wrote that the Soviet dictator “still insisted” on a base in the Dardanelles. But when one reads Smith’s record of this interview in *Foreign Relations*, one learns that the discussion actually ended with Stalin saying that he might be satisfied with much less than a base. Likewise, Kuniholm portrayed the Soviet note of August 1946 as a climactic event, clear testimony of Soviet intimidation. He did not explain that the Soviet note had been expected since the Potsdam conference. Nor did he inform his readers that, when the note arrived, there were no threats of force, no troop movements, and no military preparations. Even more interestingly, Professor Kuniholm neglected to mention that State Department officials reported that “the Turks were not particularly alarmed” on receiving the note, that the Turkish foreign minister seemed “somewhat relieved,” that the secretary general of the Turkish Foreign Office “did not seem overly concerned,” and that the Soviet note was a “less formidable blow than expected.” The second Soviet note on the straits, presented in September, was considered even softer than the first.⁸

Between August 1946 and proclamation of the Truman Doctrine in March 1947, there were few signs of Soviet pressure on Turkey. State Department records reveal one outburst of Soviet propaganda in December 1946. But in January central intelligence reported numerous signs of Soviet moderation. Edwin Wilson, the American ambassador in Turkey, informed the State Department that Molotov had gone out of his way to be agreeable to the new Turkish ambassador in Moscow. In the whirlwind of events surrounding the Truman Doctrine, Wilson maintained that the Turks were not experiencing dire financial or economic circumstances, reiterated that they expressed no fear of imminent attack, and acknowledged no threatening troop movements. Acheson concurred that no crisis situation existed in Turkey. The Army’s leading war planner informed the secretary of war that the State Department did not have the faintest idea how to justify aid to Turkey.⁹

It is worth recreating the above circumstances in some detail in order to dispel the notion that the Soviets engaged in a continual war of nerves. Let me emphasize, however, that I am *not* saying that Soviet intentions were benign or that they did not seek to enhance their interests in the area at the expense of Turkey. They

records any reference to the Soviets’ pressing the territorial issue after the middle of 1946. Among other files, I have looked at RG 59, 867.00, 761.67, 868.00, 861.20267, and 867.20.

⁸ For the comments of State Department officials, see *FRUS*, 1946, 7: 832, 835, 860, 866–67, 869. For Smith’s conversation with Stalin, see Kuniholm, *Origins of the Cold War in the Near East*, 356–57; also see *FRUS*, 1946, 6: 736. For Kuniholm’s treatment of the Soviet note, see Kuniholm, *Origins of the Cold War in the Near East*, 355–82; and for the intelligence summary, emphasizing the absence of Soviet military preparations, see Vandenberg, Memorandum for the President, August 24, 1946.

⁹ Wilson to Secretary of State, December 3, 1946, RG 59, 861.20267/11–1346; CIG, “Revised Soviet Tactics in International Affairs,” January 6, 1947; Wilson to Secretary of State, January 10, 1947, RG 59, 761.67/1–947; Wilson to Secretary of State, February 26, 1947, *ibid.*, 761.67/2–2647; *FRUS*, 1947, 5: 87–95, 109–10; U.S. Senate, *Legislative Origins of the Truman Doctrine: Hearings Held in Executive Session Before the Committee on Foreign Relations* (Washington, 1973), 50, 56; and Lincoln to Secretary of War, March 12, 1947, RG 107, RPPP, General Subject File, box 1.

clearly desired to have bases in the Dardanelles and wanted to control the straits and the Black Sea. But such goals in and of themselves were hardly exceptional. Foreign Service officers like Loy Henderson acknowledged that the Soviet quest for bases in the Dardanelles did not differ in substance from the American quest for bases in the Atlantic and Pacific. John Hickerson noted that the Soviet desire to bring the defense of the straits strictly within the purview of the Black Sea powers resembled the thinking behind the inter-American security system.¹⁰

Professor Kuniholm, then, is wrong in saying that the “driving fact in the region was inauspicious Soviet behavior” (page 388). The American conception of security in the region existed at the close of the war; Soviet behavior, while worrisome, did not constitute relentless pressure or systematic intimidation. The Truman Doctrine, after all, was not a response to any Soviet initiative but to a British note. That note portended a substantial increment in Soviet power, regardless of Soviet actions, if the Greek Left emerged victorious or if Britain departed from the region. Significantly, Secretary of State Marshall’s simultaneous decision to move ahead with new initiatives in Europe stemmed neither from Stalin’s bellicosity at the Moscow conference nor from a new set of Soviet diplomatic demands. To the contrary, Marshall’s frustration and alarm stemmed from Stalin’s patience and equanimity in the face of growing socioeconomic ferment. The former army chief of staff recognized that this ferment constituted the West’s greatest weakness and Soviet Russia’s greatest strength.¹¹

Accordingly, I find Professor Gaddis’s emphasis on the Soviet military threat in Eurasia very misleading. Soviet military capabilities did not constitute “a threat of the first order,” because neither American officials nor European statesmen expected Soviet military aggression. This was most apparent in the autumn of 1947 when American and British officials actually endorsed a partial demobilization of Turkish forces.¹² Nor did the subsequent security talks, culminating in NATO, reflect a primary concern with a Soviet military threat. In fact, on the eve of the Washington exploratory talks on security, both French and Belgian officials maintained that the Soviets had no desire to fight in Europe. Throughout the summer of 1948 British and American intelligence continued to report no signs of aggressive Soviet military intentions. In the middle of the security talks leading to the formation of NATO, Kennan “expressed disbelief that the Soviet leaders contemplated launching world conflict by military force.”¹³

Even the assumption that the Soviets had the capability to overrun Western Europe remains open to question. As I note in my essay, Eisenhower, Sherman, and other war planners doubted whether the Soviets could solve the logistical and

¹⁰ Henderson to Matthews, January 30, 1946, RG 59, Records of the Office of European Affairs, lot 54D394, box 17; and Hickerson to Henderson, December 11, 1946, *ibid.*, 761.67/12–3146.

¹¹ See, for example, *FRUS*, 1947, 2: 278–84, 337–44; Bohlen, *Transformation of American Foreign Policy*, 87–88; and Jones, *Fifteen Weeks*, 214–24.

¹² Henderson to Acting Secretary, October 8, 1947, RG 59, 867.20/10–447.

¹³ For Belgian and French views, see *FRUS*, 1948, 3: 76, 142, 152; for the British perspective, see “Intelligence Division Daily Briefing,” October 18, 1948; for American intelligence estimates, also see CIA, “The Strategic Value to the USSR of the Conquest of Western Europe and the Near East (to Cairo) Prior to 1950,” July 30, 1948, HTL, HSTP, PSF, box 256; and CIA, “Appendices to ORE 58–48,” October 27, 1948, *ibid.*; and for Kennan’s statement, see *FRUS*, 1948, 3: 157.

mobilization problems that were attendant upon launching a full scale attack. That these doubts were well founded is illustrated in the most recent and most detailed study of the postwar Soviet army.¹⁴ Thus, Professor Gaddis's allusions to Western military shortcomings without a comparable assessment of the potential adversary's weaknesses gives a distorted picture of the situation. On the eve of American intervention in Korea, the chairman of the Joint Chiefs of Staff still argued that Soviet cognizance of Soviet weakness would dissuade Russian military action.¹⁵

It is true, however, that during 1948 American policy makers came to believe that war was becoming more likely, although still improbable. American defense officials and military analysts realized that the initiatives deemed imperative to rebuild Western Europe, fill the vacuum in the eastern Mediterranean, and revive Japan might be perceived as threatening, might precipitate dangerous countermeasures, and might provoke conflict. State Department officials were no less aware of these possibilities than were the intelligence analysts cited in my essay. "The steps we are endeavoring to take," wrote Llewellyn Thompson in early 1949, "will surely increase the danger that the Russians may consider it advisable to strike before the steps can be effective."¹⁶ When American officials talked about war arising out of a miscalculation, and this was the only likely cause of war in their view, they meant that they might underestimate the Soviet perception of threat engendered by American actions or that the Soviets might underestimate the West's determination to carry out its goals even if it meant war. War, then, would arise not as a result of planned aggression, but as a consequence of a diplomatic crisis that escaped the control of policy makers.

Of course, American defense officials did not want war; they wanted peace and security. But they realized that their concept of security might be incompatible with that of the Soviet Union, especially insofar as it encompassed the rebuilding of Russia's enemies and the establishment of bases on the Soviet periphery. It was well understood, for example, that if war erupted through miscalculation, the airports and military infrastructure of Turkey, being modernized as a result of Truman Doctrine assistance, would be utilized to help launch an air attack on and eventually a land offensive against the Soviet Union.¹⁷ That Soviet leaders should have been worried about the consequences of Germany's rehabilitation, the modernization of airports on her periphery, the establishment of bases, and the formation of a military alliance in Western Europe is not surprising. Indeed, it was so understandable that American officials themselves feared that the Soviets might strike back. Yet because American policy makers defined national security as necessitating a favorable balance of power in Eurasia as well as control over Eurasian rimlands, they had no alternative but to go ahead with these initiatives. If this meant that war was more likely, it *now* became imperative to enhance one's military capabilities. Hence, in the latter part of 1948 the size of the military budget became a significant issue.

¹⁴ See Evangelista, "Stalin's Postwar Army Reappraised," 110–38.

¹⁵ *FRUS*, 1950, 7: 158.

¹⁶ *Ibid.*, 1949, 1: 293. Also see Kennan, "Remarks," January 8, 1948, RG 330, CD 3–1–36, box 8.

¹⁷ Leffler, "Strategy, Diplomacy, and the Cold War."

Does this mean, as Professor Kuniholm appropriately asks, that the United States should have been indifferent to Soviet ambitions in the Near East? Like Kuniholm, I would answer this question in the negative. But such a reply does not lead me, as it does him, to dismiss the significance of American actions and to caricaturize Soviet policies. While Vojtech Mastny had many incisive things to say about Stalin's wartime diplomacy, he did not address postwar developments. Look, for example, at the Near East. That "Stalin tightened his grip wherever it reached" (page 389) is far from clear. If he did, why did he exert only intermittent pressure on Turkey, refrain from supporting the Greek communists, and withdraw troops from Iran (when American resistance was nothing but rhetorical and diplomatic). American officials were well aware in 1945 and 1946 that they could do little to stop Soviet advances in the Near East if Stalin were determined to take advantage of opportunities.¹⁸ The problem, of course, is that we really do not know how Stalin defined his opportunities. Surely he wished to expand Soviet influence. But according to recent writers, he also did not want to rupture Soviet-American relations, sacrifice internal priorities, or weaken his leverage over communist leaders in other countries. Beleaguered by conflicting impulses, Soviet policy during 1945–48 was inconsistent, reactive, and indeterminate as well as opportunistic, pragmatic, and repressive.¹⁹

Accordingly, I would rephrase Professor Kuniholm's concluding question. The issue is not whether the Near East should have been incorporated into a Soviet or American sphere of influence but whether it might have been possible to prevent Soviet predominance and at the same time induce Stalin to define his opportunities in terms of Soviet-American cooperation. Once Stalin pulled Soviet troops out of Iran and ceased making demands on Turkey, had not American officials achieved key objectives? Yet far from satisfied, American policy makers pushed ahead to establish more modern airfields and potential bases in Turkey as well as to encourage the Iranian government to renege on oil concessions and air transit privileges previously extended to or under negotiation with the Soviet Union. Likewise, Kennan believed that the reduction of the communist threat in the Mediterranean and the end of the Berlin blockade should diminish America's military presence and lead to negotiations over Germany.²⁰ But most American civilian and military officials disagreed with Kennan. Recognizing this trend of developments, Professor Gaddis wrote in *Strategies of Containment* that the Truman administration "lost sight of the objective that strength was supposed to serve; ending the Cold War."²¹ What Gaddis failed to recognize is that strength was not designed to end the Cold War; strength was designed to achieve the national security objectives I describe in my essay, regardless of the impact on the Cold War

¹⁸ Lincoln to Embick, July 7, 1945, RG 165, ser. ABC 093 Kiel (7-6-45); Meeting of the Secretaries of State, War, and Navy, March 6, 1946, RG 107, RPPP, safe file, box 3; Kennan to Secretary of State, March 17, 1946, HTL, Elsey Papers, box 63; and Elbridge Durbrow to Secretary of State, August 5, 1946, RG 59, 761.67/8–546.

¹⁹ For postwar Soviet policies, see references in footnote 7 of my essay, page 348, above.

²⁰ For Kennan's views, see, for example, Etzold and Gaddis, *Containment*, 121–23, 135–44; for Iran, see Kuniholm, *Origins of the Cold War in the Near East*, 383–99; and, for Turkey, see Leffler, "Strategy, Diplomacy, and the Cold War."

²¹ Gaddis, *Strategies of Containment*, 118.

or on the Soviet Union. And the result of this may have been to discourage Soviet leaders from defining their opportunities in terms of a cooperative as well as a competitive relationship with the United States.

My point, however, is not to dismiss American concerns about Soviet intentions, but to illustrate the complexity of the questions that arise from a careful appraisal of immediate postwar developments. Since Soviet actions often were inconsistent, might American defense officials have developed a more nuanced assessment of Soviet objectives? Since the United States insisted on defense in depth, was it reasonable or desirable to think that the Soviets would accept anything less, especially since they suffered so much more during World War II? Since Soviet repression and communization within Central Europe sometimes escalated in reaction to American initiatives, might different American actions have elicited different behavior? Since the rebuilding of Germany, the formation of a military alliance, and the development of bases and air power obviously would be seen as threatening, could other means have been found to protect Western interests? Since postwar conditions in Asia were so turbulent, did not the American conception of security distort American requirements in that region, simplify the linkages between the Kremlin and revolutionary nationalist movements, and constrain American options? In general, might it have been possible to define and to implement American security interests in ways that might have reduced the Soviet perception of threat, aligned the United States with popular nationalist movements, curtailed the dependency on nuclear weapons and air power, and circumscribed American commitments?

Professor Gaddis seeks to dismiss the significance of such questions. Rather than grapple with the difficult dilemmas that arise from the grandiose American conception of security, he complacently writes that "we can take it for granted that Americans were not exempt from the temptations of power" (page 384). Yes, we may take it for granted, but at our peril. Taking it for granted generates incomplete accounts of the origins of the Cold War; taking it for granted retards assessments of the American share of responsibility for the breakdown of the wartime coalition; taking it for granted engenders a self-deceiving mythology of American innocence, a mythology that creeps into the work of such shrewd observers of international politics as Kissinger, Laqueur, and Gaddis himself; taking it for granted beclouds analyses of how the American definition of security clashes with the perceived security interests of other powers, thereby escalating tension and mutual distrust; taking it for granted blurs understanding of how the American conception of security generates pressures for military embroilment in areas of peripheral interest throughout the Third World.

RATHER THAN TAKE THE AMERICAN CONCEPTION OF SECURITY for granted, I suggest we continue our efforts to analyze the geostrategic, economic, political, technological, ideological, and bureaucratic factors that shape this quest for power. We need to deepen our understanding of other nation's quest for power through more

penetrating assessments of their capabilities and intentions. We must examine how the American quest for power and security can be reconciled with the similar drives of other nations, both great and small ones. And in undertaking such studies, we must look more carefully at the delicate web of relationships that tie the rivalries of great nations to regional and local instabilities. Professor Gaddis may still think these are unimportant pursuits; I disagree. Our readers can make the final judgment.

MELVYN P. LEFFLER
Vanderbilt University

Reviews of Books

GENERAL

CHARLES WEBSTER. *From Paracelsus to Newton: Magic and the Making of Modern Science*. (Eddington Memorial Lectures, 1980.) New York: Cambridge University Press. 1982. Pp. 107. \$19.95.

This short book is a modified version of Charles Webster's Eddington Memorial Lectures, delivered at Cambridge University in November 1980. As explained in the introductory chapter, Webster's overall message is the rejection of the thesis that "the rise of science was associated with a total decline of magic as it was understood in Western society in the sixteenth and seventeenth centuries" (p. 11). This message is illustrated in the next two chapters with brief surveys of the views of Paracelsus on prophecy and spiritual magic, the impact of these views on contemporaries and successive generations, and especially their influence on members of the Royal Society and the science of the late seventeenth century. Stressing that this science must not be seen as the unquestioned dominance of the mechanical philosophy, Webster points out that the persistence of the distinction between active principles and passive matter demonstrates a "continuing reference to categories bearing a generic resemblance to those implied in the basic definition of natural magic" (p. 71). Chapter 4 enters the potentially more controversial territory of the relationship between the new science and witchcraft persecutions. Because the decline of the latter "coincided exactly," Webster writes, with the rise of the former, there has been a strong temptation to assume a causal connection between the two trends, especially since it was the mechanical philosophy that generated "a new confidence in the ability of science to explain mental and physical phenomena in terms of the laws of matter and motion" (p. 88). Webster finds that this temptation must be resisted in discussing the Royal Society. Intent on distancing themselves from the materialistic implications of Hobbes's writings, its members understandably took a conservative stance on such questions as witchcraft; to the extent that attitudes

were changing in general on such matters, scientists were simply dragged along with the tide. "We must look," writes Webster, "in places other than science for the explanation of these changes" (p. 100). Yes, indeed.

The overall thesis and views presented in this book are widely accepted among historians of science. On the merit side, the detailed and compact discussion contains many references to both primary and secondary sources, although there are some surprising omissions, for example, to the pioneering work of Allen G. Debus. In criticism, Webster gives no indication that the mechanical philosophy, like its competitors, was faced with severe internal problems; the "new confidence" was not an unproblematic effect of the mechanical philosophy. The book ends exactly where interesting if controversial discussion could have begun. There is a very general criticism to be made. The "scope" of the Eddington Memorial Lectures includes the suggestion that unless the "dazzling achievements of modern science can be matched by a great moral and spiritual advance," there threatens the catastrophic breakdown of human civilization (p. v). Although the subject matter of this book is scarcely irrelevant to the problems now facing humanity, Webster nowhere ventures outside the security of his academic discipline. I find this behavior, in the context of the lectures, quite incredible.

BRIAN EASLEA
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KLAUS FISCHER. *Galileo Galilei*. (Beck'sche Schwarze Reihe, number 504.) Munich: C. H. Beck. 1983. Pp. 238. DM 19.80.

Who was Galileo? Brilliant scientist, skilled politician of science (although not, alas, as skilled as he should have been), formidable and artful polemicist, and a man with a large capacity for the enjoyment of life, Galileo remains, after several decades of increasingly detailed study, subject to the most diverse interpretations. His discoveries in mechanics have been

seen by some as derivative and by others as representing a profound and radical break with his medieval predecessors; he has been called a martyr of freedom of thought on the one hand and, on the other, one who served his science and himself ill by his arrogance; he has been variously characterized as empiricist, Platonist, and Archimedean. All agree, however, that in the course of his lifetime Galileo not only did more than most to tear down the still powerful edifice of Aristotelian natural philosophy but also that he provided abundant materials for the construction of a new, essentially modern, conception of the universe.

Our picture of Galileo's development as a scientist is clearer than it was twenty years ago. Scholars have recently shown that there is no direct line from late medieval mechanics to Galileo's early theories of motion; his starting points seem to have been rather the modified Aristotelian mechanics taught at the Collegio Romano and ideas he developed from his reading in the newly available editions of Archimedes and Euclid. Some role for experiment in the development of his ideas on motion is more commonly accepted now than it was a generation ago. Finally, we have a better understanding of Galileo's campaign to promote the new science and of the social, political, and theological context of his encounters with the Inquisition. Klaus Fischer presents these and other historiographical developments in a clear, concise, and interesting manner, *utilizing the best and most recent studies*.

Historians of science, however, will surely quarrel with certain points of interpretation and fact. For example, because the sixteenth-century physicist G. B. Benedetti "admired" the system of Copernicus, we need not necessarily infer with Fischer that he was a "follower" of Copernicus (p. 57). More serious is Fischer's assertion that Galileo would have been spared a number of blind alleys (in consequence of his early belief that freely falling bodies accelerate in proportion to distance) had he accepted the "medieval tradition" of acceleration proportional to time (p. 67). Not only would it be very difficult to show the existence of such a tradition, but it was not generally accepted until the seventeenth century that real, freely falling bodies are uniformly accelerated.

Fischer's book, however, remains an admirable summary of the chief features of Galileo's thought, the best recent studies of his evolution as a scientist, and the different interpretations of the significant historiographical issues. A useful chronology with brief biographical details is provided at the beginning of the book and there is a short, excellent bibliography. This is a useful introduction to the most important aspects of "Galileo's cognitive system, the manner of its functioning and its productions" (p. 12). It should find a ready audience

among readers of German. A work like it in English would be welcome.

WILBUR APPLEBAUM
Illinois Institute of Technology

PETER J. BOWLER. *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900*. Baltimore: Johns Hopkins University Press. 1983. Pp. xi, 291. \$25.00.

In this masterful survey of the history of evolutionary theory in the late nineteenth and early twentieth centuries, Peter J. Bowler undertakes to discover "how an ultimately successful theory may find itself overshadowed for a time by its rivals" (p. 11). The theory considered is Darwin's theory of organic evolution through random variation, struggle for existence, and natural selection. Its rivals were theistic evolution, Lamarckism, orthogenesis, and the mutation theory, to each of which Bowler devotes a chapter. There are also chapters on anti-Darwinian developments in the United States and France (involving variants of Lamarckism, theistic evolution, and orthogenesis), and an extensive bibliography.

Bowler's work is notable both for the range and depth of the scholarship and for the sophistication of the historical interpretation. His account of evolutionary theorizing in Britain, France, Germany, and the United States in the period 1880–1930 is based on careful examination of hitherto neglected primary sources, with due attention to the scientific and cultural traditions that shaped scientific thought in each nation. On the interpretive level, Bowler sets out to show that Darwin's theory, far from enjoying immediate and lasting success, faced formidable scientific opposition for several decades after an initial period of relative success. Bowler demonstrates that the evolutionary debates involved highly complex issues that left plenty of room for maneuvering within each theoretical position. The temporary eclipse of Darwinism occurred partly because theoretical deficiencies in Darwin's formulation of the theory permitted the resurgence of older modes of scientific thought permeated by German idealism and Anglo-American natural theology and partly because the new sciences of biometrics and experimental genetics were initially conceived as antithetical to each other and to elements of Darwin's theory. According to the author, the rise and fall of the Lamarckian and orthogenetic alternatives to Darwinism cannot be explained simply in terms of empirical research and logical analysis but must also be viewed in the light of "wider developments occurring in evolution theory, in biology as a science, and in society as a whole" (p. 221), and there was no one-to-one relationship between biological theories and either social or general philosophical

theories. Lamarckism and Darwinism can be given optimistic or pessimistic interpretations, and have been given both. This point is well made, but Bowler errs in describing Darwin's point of view as "radically materialistic" (p. 21). That Darwin was an evolutionary deist when he wrote *On the Origin of Species* is shown not only by his reference in the next to the last paragraph to "the laws impressed on matter by the Creator" but also by his definition (in 1856) of the laws of nature as "the laws established by God for governing the universe."

In general, however, Bowler's book is a model of thorough research guided by well-conceived principles of interpretation. It should prove interesting and helpful to everyone concerned with how science has developed in the matrix of Western civilization.

JOHN C. GREENE
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DENNIS SMITH. *Barrington Moore, Jr.: A Critical Appraisal*. Armonk, N.Y.: M. E. Sharpe. 1983. Pp. viii, 195. Cloth \$17.50, paper \$8.95.

Dennis Smith has written a lucid commentary on the ideas of Barrington Moore, Jr. but does not really convince us, as he suggests at the outset, that Moore ranks with Durkheim and Max Weber or that his admittedly pioneering analysis of the *Social Origins of Dictatorship and Democracy* merits a place on the same small shelf as *Suicide* and *The Protestant Ethic and the Spirit of Capitalism*.

The book attempts neither a serious contextualization of Moore's work—Smith's speculative and impressionistic characterizations of, for example, "the mood of the nation in the 1950s" are not likely to be satisfying to most historians—nor does it offer an independent perspective from which to evaluate Moore's historical scholarship. The concern is rather to make the argument that there is a consistent thematic and methodological evolution that can be traced across Moore's writings, to characterize that intellectual stance, and to situate it among other prominent approaches to the writing of history, sociology, and political theory.

This task is carried out through what amounts to a series of summaries of the essays in which Moore developed his theoretical approach and of the major historical and political studies in which this approach is put to work. In the case of *Social Origins* (1966) and Moore's last book, *Injustice: The Social Basis of Obedience and Revolt* (1978), we are also provided with a sampling of the controversy provoked by Moore's original presentations. A few sound criticisms are offered, such as Smith's exposure of Moore's reluctance to consider intersocietal relations in his formulaic equations, and some suggestive observations are made along the way, but the

author appears hesitant to pursue them to any real depth in this short book.

Although Smith's work thus turns out to be much more an exposition than the "critical appraisal" he promises, it does succeed in bringing into focus some of the methodological innovations that made Moore into such a distinctive and powerful contributor to postwar American scholarship. He has brought, as Smith shows, a formidable erudition and a fierce logical rigor into a fruitful alliance with the moral and political priorities that a generation of positivist scrutiny had sought to banish from scholarly research.

Barrington Moore, Jr.'s major inquiries, whether labeled narrative history, comparative sociology, or political theory, have been motivated by a concern to extract universal generalizations about human behavior from intensive historical case studies of representative societies. "Misery" and "injustice," normative concepts whose meaning can only arise out of a series of subjective judgments on the part of the analyst, figure at least as prominently in his work as the more apparently objective categories of "dictatorship" and "democracy."

Although a certain intellectual idiosyncrasy hovers about the writings of Barrington Moore, Jr.—an idiosyncrasy that has bedeviled Marxists, Structuralists, and others, whether they be admirers or critics—he is forcefully articulate and quite unambiguous in establishing his personal goal of integrating the analytical and the normative within a single narrative structure. Consequently, the reader of Smith's book, who may be grateful for a clear reaffirmation of Moore's method, soon becomes impatient with a commentary that does not press further to engage his ideas.

BARRY M. KÄTZ
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GERHARD OESTREICH. *Neostoicism and the Early Modern State*. Translated by DAVID MCLINTOCK. Edited by BRIGITTA OESTREICH and H. G. KOENIGSBERGER. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1982. Pp. viii, 280. \$49.50.

Gerhard Oestreich (1910–78) intended to write (in the words of the editors of this volume) a "grand, overall history of early modern Europe"; moreover, he would probably have liked to bring his specialized studies on Lipsius and neo-Stoicism together into one coherent synthesis. The organization of his life or the structure of his mind, however, prevented him from realizing these major projects. In 1969 he decided to publish a collection of twelve articles on Lipsius and on early modern constitutional history in a volume called *Geist und Gestalt des frühmodernen*

Staates, followed after his death by a second installment, *Strukturprobleme der frühen Neuzeit* (1980), containing sixteen studies. The book under review presents an English version of fifteen of these articles, all of them revised by the editors. It is perhaps because of this that the editors do not indicate the original dates of publication; nor do they specify the purposes for which they were written. This is a pity. Oestreich was generous with his time and energy, and a large number of his essays were written for collective works or symposia where he was invited to summarize his views. This explains why he sometimes felt obliged to repeat himself; it was not because he thought his discoveries so important that he could not stop rehearsing them.

This, however, is a minor point. The book is a success as it is, well translated, scrupulously edited, and worth having. We find here most of Oestreich's essays on the political implications of neo-Stoicism in the form into which it was cast by Justus Lipsius. Oestreich's views on this subject were pertinent. He attached a great deal of importance to the practical philosophy taught at the universities of the seventeenth and eighteenth centuries and thought that it constituted a decisive element in the growth of the absolute state. As Lipsius taught from 1579 to 1591 in the University of Leiden and later at Louvain, and as both in the northern and the southern Netherlands he gathered a loyal group of admirers and disciples around him, Oestreich characterizes Lipsius as the initiator of a "Netherlands Movement." The first half of this volume is devoted to the analysis and description of this movement and of all its ramifications in the European academic and political world. It is to be hoped that Oestreich's views will now be more broadly discussed. They are coherently argued and most thoroughly documented and may serve as starting points for new research with perhaps different results.

The second part of the book contains learned articles on the origin of representative government, the social contract, the German estates, and other themes. They form less of a unity than the first half of the collection but it is good to have them in English for they are thoughtful, useful, and erudite.

E. H. KOSSMANN

University of Groningen

MARC RAEFF. *The Well-Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600–1800*. New Haven: Yale University Press. 1983. Pp. ix, 284. \$23.50.

In the three essays that make up *The Well-Ordered Police State*, prominent Russian historian Marc Raeff attempts, through a comparative study of the Ger-

man principalities and Russia, to define the nature of the political attitudes and administrative practices of the seventeenth and eighteenth centuries and to show their central importance in shaping the modern European society that would impose its pattern on the rest of the world in the nineteenth and twentieth centuries. Raeff's conception of this pattern is essentially Weberian: secular, functionally rational, production-oriented, dynamic, forward-looking, materialistic, and atomized. Weberian, also, is Raeff's view that the actualization of this pattern—in the "well-ordered police states" of the Germanies and Russia, as elsewhere—was largely the work of political authorities and elites whose "enlightened" legal and institutional reforms effectively legitimized the movement toward modernization they sought to promote. Raeff finds that, in the Germanies, where sovereigns could count on the cooperation of already constituted groups such as guilds, estates, municipalities, universities, and communal jurisdictions, the state played a major role in creating the legal and institutional framework that made possible a gradual and relatively peaceful transition from traditional to modern society. In Russia, where few such groups existed, the transition was erratic and violent, and the reforms of Peter I and Catherine II succeeded mainly in strengthening the hand of the central state against a society resistant to modernization.

In the first essay, Raeff sketches the intellectual transformation of Europe—the Renaissance and Reformation, the Counter Reformation and religious wars, the rise of science and rationalist philosophy, and the appearance of a literature of utopias—that formed the framework and defined the purposes of practical legislation. Sovereigns were encouraged to believe that, by organizing productivity and by codifying and enforcing the laws, they could reshape public behavior and social action so as to increase the wealth and power of the state and the welfare and happiness of its subjects.

The second, and longest, essay examines the efforts of the rulers of the German principalities to achieve these goals by enacting a mass of often repetitive and overlapping ordinances and regulations to control and streamline virtually every aspect of life: religion, culture, commerce, guilds, education, public and family events, manners and dress codes, and the administration of the state itself. The ordinances aimed to increase production and curtail consumption; the method used was to discipline a still deeply traditionalistic society (not only in its outward behavior but also in its attitudes) into accepting these new goals, which Raeff considers the truly innovative aspect of the ordinances. Those whose behavior and activities were perceived to be contrary to a society organized for production—Jews, gypsies, beggars, and vagrants—were dealt

with harshly and isolated from the rest of society. Following Foucault, Raeff reasons that "the structured productive society had to put everybody into a rigid institutional setting, not only to control them but also to preclude their becoming dangerous models for others" (p. 90). The disciplining of attitudes was effective, and in the German states the rate and thoroughness in internalizing the values and norms of the new production-oriented political culture was significantly higher than in Russia, which is the subject of the third essay.

Raeff describes the Russian power structure as a "one-way or one dimensional affair" (p. 186), due to the dearth of powerful corporations and autonomous groups that government could use, as in the German states, to discipline and modernize society. Peter I's overpowering personality and driving energy and Catherine II's partial success in creating productive elites loyal to the state enabled these autocrats to make some headway in modernizing Russia. But though they tried to imitate the West—Catherine more successfully than Peter—they actually reversed the model, by making private activities dependent on state needs and service. When the "well-ordered police states" that had initiated modernization in the West were replaced by more representative political forms, society's capacity and urge to produce and profit were enhanced. The collapse of the Russian government in 1917, by contrast, left a void that would be filled by a radical intelligentsia with new ideas about how to organize society and its productive forces.

Some historians, especially of the Marxist and *Annales* variety, may take issue with the emphasis Raeff gives to legal and administrative innovations in the formation of modern society. Others may feel he is too uncritical of the aims and achievements of the "well-ordered police states," and that he overstates the structural differences between the German states and Russia from 1600 to 1800. But what must be acknowledged is the careful, thorough research and the thoughtful arguments on which Raeff builds his case.

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DEREK MCKAY and H. M. SCOTT. *The Rise of the Great Powers, 1648–1815*. New York: Longman. 1983. Pp. xi, 378. Cloth \$30.00, paper \$13.95.

This work on European diplomatic history from the Peace of Westphalia to the Congress of Vienna is an excellent example of a difficult genre, the high-level textbook. Because it serves the needs of advanced students and, indeed, of their teachers, it must be

abreast of the literature of the field, past and present; yet it must present a unified and intelligible picture of extraordinarily complex events. Variety and simplicity contend with each other, yet the victory of either would be disastrous. The problem is made more difficult for a diplomatic historian because of the multiplicity of languages that must be used. In this book, all of these demands are successfully met; that some shortcomings remain will seem quite venial, certainly to anyone who has ever ventured on a similar project. It is not at all a flaw, however, that the very large picture painted is one long-familiar to historians; rather, Derek McKay and H. M. Scott are to be complimented for not seeking novelty for its own sake when the established very broad outlines have been confirmed by continuing scholarship. The details are often new, changing the shading, the colors, and the precise contours. In the end, the picture is both familiar and fresh.

The authors are also to be praised for continuing to see the task of diplomatic history as the study of the development of the states system, neither washed out into a broad history of intercultural relations nor diminished to the history of each country's foreign policy; international relations are properly seen as shaped by the interaction between domestic concerns and the needs of states as such. The demand this makes on the knowledge of a historian studying the relations between only two countries over a relatively short period is immense enough to daunt all but a few historians; when two historians are required to command the history of a score of states, more or less, over two centuries (for the authors take a long running start to the beginning of the Thirty Years' War), they cannot help but slip. This reviewer observed this happening in their account of Dutch institutions and events, admittedly a quagmire that has swallowed more than one venturesome historian before them; yet I suspect that readers with specialized knowledge in other areas will find the same failing.

What is more important than such mistakes, however, is how little they affect the large picture, at least in the cases where my own knowledge is strongest. To insist on more than McKay and Scott have achieved would be to demand the virtually impossible and the probably unnecessary. I wish I could say the same for the prose in which their book is written. It is surprisingly rough and careless, taking a shade off our general pleasure in reading a work of such range of knowledge, subtlety of analysis, and wisdom of judgment. Advanced scholars and advanced students alike will still find it a book from which they will learn much.

HERBERT H. ROWEN
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CLIVE H. CHURCH. *Europe in 1830: Revolution and Political Change*. Boston: George Allen and Unwin. 1983. Pp. xiii, 210. \$22.50.

This book is a welcome addition to the literature on nineteenth-century protest. As Clive H. Church notes, no one has previously attempted an overall survey of the revolutions of 1830. The present effort is competent and comprehensive. Intended for classroom use, it demands no special preparation and offers correspondingly few surprises. But many historians will find the factual summaries useful.

Church largely avoids the temptation to claim too much for his subject. Although he sets up a few straw men, insisting that the 1830 revolutions were not superficial affairs instigated by a few outside agents, he admits that the revolutions do not constitute a "supervent." His assessment of their long-range impact is sound, though somewhat truncated as far as certain forces—such as the church or the politicized artisanry—are concerned.

The account of the revolutions themselves is solid, based on substantial secondary reading plus some consultation of primary materials on Belgium, Switzerland, and Italy. The author balances social analysis and political-diplomatic narrative, though with a preference for the latter. Social forces emerge rather vaguely, except for a good delineation of middle-class involvement. Most historians will find the treatment of Belgium, Switzerland, and central Italy most interesting because less accessible in commonly available literature. Sections on Poland and Portugal are helpful as well. Church covers France only briefly, reacting against the too facile use of the country as revolutionary exemplar; for novice readers, he downplays the French case excessively, though major developments are covered. Brief chapters also treat agitation and reform in Northern Europe, Spain, and the Balkans, at some cost to analytical coherence, as the account is torn between a focus on the revolutions of 1830–34 and an effort to convey more general political developments in the same years.

Church is least compelling as he grapples with the problem of fitting 1830 into a model of revolution. Although he usefully deals with the successive waves of revolution as agitation spread south and east, he also insists on the individuality of each rising. As a result, general typologies are lacking. Correctly noting that the revolutions do not fit the model dynamics of risings like 1789 or 1917, Church eschews any other framework. Oddly, comparisons with the other wave of brief revolutions in the nineteenth century, 1848, are avoided; the exciting possibility of comparing 1830 to epidemic revolts in the non-European world in the twentieth century is entirely ignored. Obviously, one could have hoped for more

conceptual daring, particularly given Church's stated desire to inspire further work on his subject.

Despite some shortcomings, the book fills a need, accurately summing up existing knowledge confined until now to national accounts, adding a dab of social and cultural history, and above all providing the best available geography of what was a significant revolutionary outbreak.

PETER N. STEARNS

Carnegie-Mellon University

THOMAS P. HUGHES. *Networks of Power: Electrification in Western Society, 1880–1930*. Baltimore: Johns Hopkins University Press. 1983. Pp. xi, 474. \$38.50.

Judged by its full title, *Networks of Power* would appear to be a ho-hum story of the growth of electrical power systems in the United States, England, and Germany in the years between 1880 and 1930. This work, however, is not some dreary narrative, written for specialists who lisp in numbers. Rather it is an exciting, major contribution to the field of history, for it establishes very convincingly that the growth of such power networks is as intrinsic to and characteristic of modern society as the growth of manorialism was to medieval society. Thomas P. Hughes successfully demonstrates that the growth of such cultural features can be as insightful and vital a theme for the analysis of Western society from 1880 to 1930 as the growth of railroad systems is for nineteenth-century American history. In addition, as a result of this research, the historian now has available a far better means of conceptualizing a Second Industrial Revolution. Hughes writes authoritatively, with exhaustive documentation, and lucidly, studying a wide variety of environments—London, Berlin, Chicago, California, Pennsylvania, New Jersey, Bavaria, and England.

The significance of this work does not rest only on the definitive manner in which the immediate subject is set forth. One prime contribution is Hughes's formulation of a general model to describe the growth of technology and its supporting institutions. Another important contribution is to the methodology of modern history. Hughes's work makes clear that, as a system grows and matures, it becomes more diverse; it generates new properties and powers, and new critical problems have to be solved. In effect, the nature of the system changes as it grows, and so, at the different stages, any historical study must describe it and analyze it in different ways. As the system grows from laboratory to urban, regional, and finally national levels, the interplay of different factors—personal and institutional, political and legislative, business and financial, geographical and economic—takes on different forms; the

factors interact in different ways and lead to different results. The author exploits this diversity of historical forms with considerable skill.

Because Hughes has formulated a general model for the growth of systems, independent of technological and local features, he can now distinguish among such cultural artifacts by means of what he calls "style." What is common to all systems is the pool of technology. What is *not* common is the particular mix resulting from the selection from that pool by a certain environment; the resultant features constitute the style or individuality of a given system. The development of such features is comparable to the process of speciation in natural history. Because style does have this contingent reality, bearing the marks of its birth, it is a useful tool for the historian.

Above all, *Networks of Power* is a history: all the narratives in the work are embedded in and supported by a historical context that answers the vital question "why," as well as the prosaic question "how." The author shares with the reader his understanding of cause and effect, although the reader is never bludgeoned with it; nor do the facts have to slumber in any Procrustean bed. The field of research here has been exhausted for a while; as for the interpretations, they will provide a sound and stimulating basis for further research. I fervently hope that Hughes will next turn his attention to the non-barking-dog case of French technology and society, and to the application of his studies and methods to the scene of Western society during the troubled 1930s.

The color and nonglare quality of the paper, as well as the care shown in the format of the book, add much to its readability. There are many appropriate and choice illustrations to supplement and enliven the text. It would be a serious mistake to be put off by the few technical terms in this book; it is a valuable and significant contribution to the literature of history, both in substance and in methodology.

W. JAMES KING
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JUDITH M. HUGHES. *Emotion and High Politics: Personal Relations at the Summit in Late Nineteenth-Century Britain and Germany*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xi, 232. \$28.50.

Judith M. Hughes's latest book is an intriguing and unorthodox study—as she puts it, "resolutely and uncompromisingly individualistic" (p. 3)—that sets out to shed new light on why the national leadership of Britain and Germany increasingly failed to understand one another after 1880 or so. Eschewing

traditional theories relating to politics, economics, and the European balance of power, Hughes focuses her attention on certain personal relationships that, she feels, both reflected and contributed to the "dissonant tempers" held in London and Berlin. Because the leaders of the two countries missed "each other's mental track" (p. 210), the increasing antagonism was not to be wondered at.

While *Emotion and High Politics* covers familiar territory, it is essentially a work of psychological rather than political history. In the author's view, the root cause of Anglo-German misunderstandings lay in the childhood experiences of select members of the two elites and, in particular, in their "contrasting experiences of maternal care." Despite all the individual differences between Bismarck, Kaiser Wilhelm II, Holstein, Eulenburg, and Bülow in the one country, and between Gladstone, Salisbury, Chamberlain, and Balfour in the other, a common national pattern was evident: the German "temper" was fearful, fatalistic, and suspicious; the British "temper" was scrupulous, trustful, and collegial, which was not only a prerequisite for civilized cabinet government but also indicated the "good enough mother" characteristics of the system.

No doubt traditional political and diplomatic historians will, like this reviewer, swallow hard at such an Oedipal interpretation of Anglo-German relations; their doubts may well be reinforced by Hughes's dedication of *Emotion and High Politics* to her son and by her candid admission that her own maternal preoccupations affected the vision of this book. Before rejecting this unorthodox approach outright, however, they ought to note the positive and attractive features of her work. It is extremely well written, and only very occasionally does it sink into the dreadful jargon of the psychological textbook. It has some very perceptive things to say on interpersonal relations among British politicians, for example, on the remarkable harmony between Salisbury and Balfour and on the civilized way in which Gladstone regarded dissent among his fellow cabinet members. (By contrast, the portrayals of relationships in Berlin seem a heavy-handed form of psychohistory, even when dealing with such curious characters as Holstein and Eulenburg.) And, finally, it does seem entirely plausible to contend that the formative years of a statesman's life *could* affect his later politics, although the argument for any such casual link needs to be properly supported by evidence and not merely conjectured in a general way.

Here, alas, is the rub. For the fact is that, however interesting Hughes's remarks on the early lives of such statesmen may be, the *connection* between their maternal experiences and the later course of Anglo-German relations is neither pursued nor explained. The reader (even one sympathetic to this project)

will simply find himself at the end of the book with an utterly self-contained argument, namely, the "contrasting experiences of maternal care" in Britain were different from those in Prussia-Germany, ergo the two governments failed to understand each other. At no point in the tale does Hughes introduce the popularly ascribed causes of Anglo-German discord, such as the colonial, trade, and naval rivalries, and suggest how they related to the (in her view, more fundamental) clash of tempers; indeed, in her preface, she deliberately proclaims her intention of distancing her work from familiar, empirical, topic-oriented approaches. In consequence, the reader is presented with the stark choice of accepting or rejecting her claim that Anglo-German misunderstandings were significantly affected by deeply rooted, pre-Oedipal elements.

Accepting such a claim seems, to this reader, impossible—not because Hughes's hypothesis is *ipso facto* absurd, but because it is not substantiated precisely at the point where the author needs to prove its causal linkage. The whole approach, in fact, begs some quite vital questions about historical causality. For example, is Hughes implying that, had the childhood experiences that produced the "dissonant tempers" on each side of the North Sea *not* existed, there would also have been no consequent Anglo-German antagonism despite the economic, ideological, and geopolitical trends that, in the view of other historians, were driving the two countries apart? How, in light of her approach, does one explain the often heated quarrels between "pro-Germans" and "anti-Germans" in Edwardian Britain, or between "Anglophobes" and "Anglophiles" in Wilhelmine Germany (that is, *internally* divided "mental tracks")? Can this be done by reference to the variegated childhood experiences of the disputants or, instead, to their ideology, social position, and so forth? Finally—and this is a dimension not taken up by Hughes, even to dismiss it—in what way does this theory of childhood-induced "dissonant tempers" fit into the story of the relationships that Britain and Germany had with *other* countries? Can the twenty-year-old misunderstandings between London and Paris after 1882, followed then by the famous *Entente Cordiale*, also be explained by this maternal, Oedipal theory? If not, why not? If yes, how does that argument go? And was the parallel lack of Austro-Russian harmony also rooted in their leaders' early upbringing? Her book makes claims, implicitly if not explicitly, for the theory of "why nations differ" that need to be confronted by questions such as this.

In the preface, Hughes expresses the hope that the purpose of her study, though controversial, will be recognized and appreciated. Recognition may come easily, given the welcome boldness of her approach; but appreciation of this book is less likely

to be rendered by fellow scholars, or at least by those who will feel that the link between psychological factors and political consequences has been stated but not substantiated here.

PAUL KENNEDY
Yale University

DENNIS B. KLEIN. *Jewish Origins of the Psychoanalytic Movement*. (Praeger Special Studies; Praeger Scientific.) New York: Praeger. 1981. Pp. xvi, 198.

This book collates much bibliography on Otto Rank and, especially, Sigmund Freud's relation to Jewishness, including newly revealed documents on Freud's first airing of his developing ideas on psychoanalysis before the B'nai B'rith chapter in Vienna, especially from 1898 to 1902. The conceptual results of this new information are meager indeed, mainly because Dennis B. Klein never reaches a rigorous definition of the key modifier in the book's title, "Jewish." Klein uses various vague terms—"Jewish feeling," "Jewish heritage," "humanistic Judaism"—without defining them. To Klein, "Jewish consciousness," a term used repeatedly, never entails Jewish rituals, texts, languages, or doctrines. Klein uses "Judaism" itself to grace Rank's bizarre definition of Judaism *per se* as a stress on sexuality. The list of vague or inappropriate terms runs much longer. The book suffers from errors of elementary fact: the Talmud is not, as Klein says, written in Hebrew; Klein's listing of the major Jewish holidays wrongly includes Purim and wrongly excludes Sukkot and Shavuot.

Following a chapter on the transition from liberalism to antisemitism and from assimilation to dissimulation in Vienna and before his chapter on Rank, Klein cites essentially two incidents and one letter written by Freud in the mid-1880s and then skips to Freud's involvement with B'nai B'rith beginning eleven years later. On this evidence Klein argues that psychoanalysis was "Jewish" because Freud, in adopting it, learned not to be ashamed to call himself a Jew, because Freud sought to cure neuroses he perceived in Jews (neuroses that resulted from what he saw as Jews' obsessive attempts to overcome marginality), because Freud first aired his views on psychoanalysis for some years before a Jewish audience, and because all of Freud's first associates in the psychoanalytic movement were Jewish. Klein sees the latter two points as particularly significant. They reflected Freud's awareness of antisemitism, his need to associate with Jews, his emergent unwillingness to hide the fact that he was a Jew, and, finally, his view that psychoanalysis was, in Klein's words, a "Jewish mission of redemp-

tion"—another variant of the same climate of disappointment in assimilation that produced Theodor Herzl, Otto Weininger, and others.

Klein's line of reasoning (even if there were no gaps in the evidence) highlights the need to distinguish methodologically between substantively Jewish matrixes—language, text, ritual, doctrine, and political sovereignty—and various conditions of suffering, marginality, and discrimination imposed on Jewish people by others. Unable to make this distinction, Klein has talked not at all about *Jewish* origins of the psychoanalytic movement. Simon, Bakan, and others have done that (accurately or inaccurately). Klein has simply argued that to undergo a change in response to antisemitism, and as a result of that change to apply the term "Jewish" to one's thinking or to one's self, *ipso facto* makes it so. With this minimal and ephemeral definition of Judaism and Jewishness, Klein's vagueness in basic terminology becomes a necessity.

HILLEL GOLDBERG
Hebrew University

ARTHUR E. IMHOF. *Die gewonnenen Jahre: Von der Zunahme unserer Lebensspanne seit dreihundert Jahren oder von der Notwendigkeit einer neuen Einstellung zu Leben und Sterben*. Munich: C. H. Beck. 1981. Pp. 279. DM 38.

Arthur E. Imhof, a social historian at the Free University of Berlin, has risen to international importance during the past five years. He exemplifies the interdisciplinary tendencies, allied with quantitative methods and demographic interests, that are characteristic of the "new" school. Although not a medical historian, Imhof takes up subjects that used to be the preserve of that specialty, and he has been honored by an invitation from the American Association for the History of Medicine to deliver the 1984 Garrison Lecture.

Three central tasks are set forth for this book (dedicated to the late Andrew B. Appleby). The first is an effort to clarify the means by which information about vital characteristics in the lives of average persons since the seventeenth century can be obtained and utilized. This goal is forwarded successfully in a forty-four page essay concerning the use of computers to organize, from local parish registers, data about births, deaths, and marriages. Such data, for example, are shown to indicate the increasing use of primal means of birth control in correlation with the declining mortality rates among infants and mothers.

The second major aim of the book requires by far the greatest attention and space: this is the spelling

out of "what basically has changed" during the past several centuries in regard to the increase in life expectancy. Nearly all the sixty-two graphs (and four charts) bear on these questions, and the text, by and large, consists of exposition and commentary related to the wealth of demographic and epidemiologic illustration. The fact that the most dramatic improvements occurred in younger age groups is well known, but, nonetheless, the graphs and discussion bring home to the reader, with impressive power, just how drastic the changes have been. It should be noted that, although most of the material is German, the author frequently branches out to surrounding countries, thereby acquiring a general European viewpoint.

Imhof is both comprehensive and detailed in his review of a long series of variables, each in its turn, such as place of residence, occupation, sex, social class, and so on. There is a considerable analysis of the evolution of the "biodemographic life course" of women. The work concludes with a summary of the demographic transition, that is, the metamorphosis of the overall population age configuration (traditionally pyramidal) and a concise survey of the epidemiologic transition, that is, the transfiguration of the "spectrum" of causes of death. Comparisons are made indicating that the Third World at present is passing through demographic and epidemiologic developments that took place earlier in the West.

In dealing with his third goal, the need for a new attitude about life and death, the author turns out to be surprisingly diffuse and ambivalent. In a work he edited in 1978 (*Biologie des Menschen in der Geschichte*), Imhof was very straightforward in his views, which seemed to me identified with those of the "counterculture"—that is, he was critical of establishment institutions and suspicious of biomedical scientism. In the present work, despite its title, these themes are much less specific. The antagonism to establishment institutions appears mostly in the form of emphasizing the "common man" (average persons and populations) and generally avoiding the efforts of organized medicine. The antiscentism is expressed largely in a sort of equivalence of gains and losses that recurs in the text: for example, there are fewer orphans now but more children with divorced parents. But another motif takes a greater role—and that is Imhof's historicist belief that by presenting the facts in a clear and precise way, in the perspective of the past, it will become evident to the reader that new attitudes are needed, and clues are provided as to what these new attitudes might be. Imhof also points out a number of ways in which, without conscious action, basic attitudes already are in a process of transformation, reflecting the altered experiences of the populace.

Imhof's historicism is attractively presented; I was impressed also by his sensitivity to the feelings and

struggles of the people represented in his data. He seems to have a genuine perceptiveness to the drama in the everyday vicissitudes of ordinary folk. It is reassuring to read quantitative history "from below" that really becomes vivid, moving, and even eloquent. Moreover, Imhof's style often is appealing in its modesty, directness, and, at times, humor. It would be commendable if a publisher were to bring out an English translation.

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EPHRAIM H. MIZRUCHI. *Regulating Society: Marginality and Social Control in Historical Perspective*. New York: Free Press or Collier Macmillan, London. 1983. Pp. xi, 207. \$29.95.

The subject of this cross-cultural analysis is not the formal "regulation" that historians usually examine, but a specific aspect of the social control function posited by sociological theory. Ephraim H. Mizruchi's conceptual contribution is the definition of this specific "abeyance function," which reduces social disorder or strain by removing excess competitors for available "status vacancies." The mechanisms performing this function are formal or informal institutions that provide socially recognized but non-essential roles for persons who might otherwise aspire to scarce positions, until vacancies become available.

Mizruchi cogently points out that "the more diverse in time and place are the contexts to which theory applies the more convincing is the theory" (p. 9). Consequently, he considers two medieval phenomena, monasticism and the Beguine movement, as well as two cases ("bohemianism" and "education") that span the modern period and the chronologically restricted record of the New Deal Artists' and Writers' Projects. A historicist approach could justify such limitations of the examination to Western civilization as a method for getting at the dynamics of a unique process. True to his functionalist assumptions, however, Mizruchi does not try to show any evolutionary relationship among his chronologically sequential cases, although he does suggest that repetition of a specific institutional pattern, such as bohemianism, is conditioned by historical precedents. Given this approach, a broader range of cases (with greater economy in presenting each) would have more convincingly illustrated the universal (nomothetic) applicability of the abeyance function.

Most of Mizruchi's cases are plausible. The discussion of monasticism appears strained, though, not because of any historiographical defects but because it fits uncomfortably into the author's general

framework—as his tabular overview (p. 25) suggests. Monasticism is far longer lived and more formally hierarchically structured than the other institutions considered, and its adherents have a much more sustained commitment. During its notable expansion in the tenth and eleventh century, Mizruchi argues, monasticism had the latent function of controlling restless, overzealous elements of a growing European population. The conventional interpretation, that the Cluniac revival provided the essential mass of religious for curial centralization and ecclesiastical reformation, remains more plausible. Mizruchi argues very reasonably that, after the Black Death, middle-class and upper-class women who had little prospect of "suitable marriages" temporarily entered the Beguines. But he does not explain how this demographic contraction was compatible with continued flourishing of the mendicant orders, which had originated during the peak of European population growth. To me, these discrepancies suggest that the abeyance function, while essential for the Beguine movement, was a rare and transitory feature of monasticism.

Such specific difficulties point to a more fundamental problem for functionalist analysis in history—the tendency to assume rather than to investigate the independent variable, in this instance, insufficiency of status vacancies. To be sure, Mizruchi does not contend that status vacancies were always directly related to gross demographic trends, although at times (as in his dubious contention that the radical "bohemianism" of the American 1960s resulted from too many people for available positions) he comes close. Nevertheless, any serious effort to extend his generalization to the medieval period would require resort to standard demographic treatments like those of Julius Beloch or recent *Annales* investigations to establish a base line for estimating pressures on specific status categories. As the author emphasizes, however, his contribution is to explore a new conceptual perspective. One may hope that his insights will lead other scholars to take a fresh look at their specific materials.

JOHN A. ARMSTRONG
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JAMES A. BOON. *Other Tribes, Other Scribes: Symbolic Anthropology in the Comparative Study of Cultures, Histories, Religions, and Texts*. New York: Cambridge University Press. 1982. Pp. xiv, 303. Cloth \$34.50, paper \$10.95.

James A. Boon discusses in a holistic manner the ways in which culture has been analyzed and symbolized by thinkers ranging from the Jacobean and Darwin to Peirce, Saussure, and Geertz. Unfortun-

nately, the historical chapters of this book seem least authentic and firsthand. The interpretations of Balinese society, the source of Boon's field work, seem more directly engaging, though of less interest to the historian. As the author admits, he makes far too much use of secondary works and armchair anthropologists (for example, Durkheim, Mauss, and Weber). Even his attitudes to major figures in the history of sociology and anthropology are often derived from secondary authorities rather than from close reading of the texts. His secondary authorities include the major culturological figures on the American scene and the European structuralists, but Boon could have added recent works in his field by younger interdisciplinary scholars in Europe: Malcolm Crick's *Explorations in Language and Meaning* (1976), Fritz Kramer's *Verkehrte Welten* (1977), and my own *Culture in Comparative and Evolutionary Perspective* (1980). *Other Tribes, Other Scribes* never obscures its origin as a collection of essays, some previously published, and graduate seminars. More than a book, it is a set of rambling disquisitions, filled with overly long quotations strung together, long parenthetical remarks, and other, less well-marked, digressions and diversions.

The integration that the historian seeks is lacking. Some recurrent themes appear: one cannot be objective in one's view of another culture, since one views it from a certain culture; cultures are dialectical (there is no culture, only culture and counterculture); there is no uniform human nature; religious and partisan political and regional interests affected European interpretations of the exotic in the seventeenth century; the Enlightenment influence is to be removed from anthropology and replaced by a cross-cultural Augustinean-Coleridgean-transcendentalist-pluralist "dramatism" of polar meanings.

The author says that structuralism must be exogamous and polygamous (p. 228). In this case, the marriages have produced offspring whose relationship is difficult to trace except by those initiated into the rites of the "dia-tribe" and its clans.

JOAN LEOPOLD
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ORLANDO PATTERSON. *Slavery and Social Death: A Comparative Study*. Cambridge: Harvard University Press. 1982. Pp. xiii, 511. \$30.00.

Orlando Patterson envisages this volume as a response to the classic study of slavery by H. J. Nieboer (*Slavery as an Industrial System: Ethnological Researches*, [1910]). Nieboer concentrated on slavery's external relations in preliterate societies, and Patterson sees his primary challenge as the examining of the internal relations of literate societies. The

Harvard sociology professor has responded brilliantly to the challenge and his book will be the point of reference for most studies of slavery for the next several decades.

Patterson sees the "institutionalized liminal state of social death" as the unifying theme of slavery in all societies. This is akin to concepts employed by others, such as the slave as "an outsider" or slavery as an "institution of marginality," which Patterson also uses. Borrowing the language of sociobiology, Patterson also considers slavery as "human parasitism."

The core of the book concerns the basic social facts of slavery: how people have become slaves throughout history; their condition (naturally alienated, culturally dead, without honor) and treatment while they were slaves; and how people ceased to be slaves through various forms of manumission (symbolic rebirth). Slavery is interpreted as a relation of domination rather than as a category of legal thought (although law is not ignored). A chapter on the eunuch as the ultimate slave is exceptionally insightful. Vast numbers of societies have been examined, from those catalogued in the Human Area Relations Files to the more well-studied ones of classical antiquity (presented most thoroughly) and the circum-Caribbean world. Noteworthy is the addition of Korean slavery to the pool of systems discussed. Probably half of the literature Patterson uses did not exist when he commenced studying slavery. His digesting and reporting of this vast body of information is generally unexceptional, although readers interested in the facts about slavery in Muscovy would be well advised to read my *Slavery in Russia, 1450-1725* to verify his presentation of that material.

Although the erudition represented in *Slavery and Social Death* is truly prodigious, even more impressive is Patterson's utilization of symbolic anthropology, philosophy, theology, law, psychology, economics, statistics, and other disciplines in his analysis. This is a model of scholarship for any comparative discussion of institutions. Patterson's systematizing (seven major rules of inheritance of slave status, five systems of slave acquisition, six types of slaveholding societies, six causal patterns of manumission, and seven modes of universal forms of release) should be a major boon to readers with broad perspectives, such as world historians.

Some of Patterson's dogmatic remarks about central tendencies (sexual exploitation [pp. 6, 173, 229]; responsibility for slave delicts [pp. 22, 196]; the *peculium* [p. 182]; the *servus vicarius* [p. 184]; the right of slaves to engage in commercial transactions [p. 197]; marriage as a producer of free status [p. 229]; and the status and obligations of freedmen [p. 241]) certainly are not as universally applicable as he states.

Harvard University Press is to be congratulated for publishing an aesthetically pleasing book at a reasonable price. The footnotes will be "must reading" for many and should have been placed at the bottom of the page. Patterson's synthesis will be the standard work on slavery, and thus the absence of a bibliography is genuinely lamentable; this flaw is compounded by the frequent practice of not listing authors mentioned in the text in the footnote citations.

RICHARD HELLIE
University of Chicago

ANCIENT

JOHN VAN SETERS. *In Search of History: Historiography in the Ancient World and the Origins of Biblical History*. New Haven: Yale University Press. 1983. Pp. xiii, 399. \$30.00.

This provocative book grew out of a faculty seminar held in 1974–75 by the department of Near Eastern studies at the University of Toronto, on the subject, "Histories and Historians of the Ancient Near East." Three of the seminar papers, including that of John Van Seters, were later published in *Orientalia*, volumes 49 and 50, along with a preface by the seminar chairman, John W. Wevers, who reported the questions the group had hoped to answer: How did ancient writers deal with the past? What kinds of materials did they use? What did they think to be history and the stuff from which history is made? "We were interested," wrote Wevers, "in what the ancients thought to be history rather than in history itself. In fact none of us is quite certain as to what history is, though we all believe that we can recognize historical materials."

In line with those remarks, which account for the book's title, Van Seters accepts as the guide for his work a definition of history proposed in 1963 by Johan Huizinga: "History is the intellectual form in which a civilization renders account to itself of its past" (p. 1). But Van Seters unaccountably reads for "civilization," "nation" and finally misquotes Huizinga to state that history is "an intellectual form whereby a people render an account to itself of its past" (p. 320). Indeed, he concludes his book by asserting that "the *sine qua non* of history writing" is "to communicate through this story of the people's past a sense of their identity" (p. 359); and he claims that this is possible without reference to either their philosophical or theological presuppositions. He asserts, for example, that "no cyclical view of time is evident in the Greek histories, whatever the philosophers might say, and there is no eschatology in the Israelite histories, whatever prophet and apocalyp-

tist might propose" (p. 8 and following). He seems not to realize that "history" represents a particular way of looking at life (see T. S. Kuhn's discussion of the role of paradigm in human thought in his seminal work, *The Structure of Scientific Revolution* [1970]).

With all his evident scholarly erudition and vividness of description, Van Seters seems unclear about the basic purpose of his book. The two parts, 208 pages on ancient historiography and 153 on the Hebrews, do not clearly relate to each other; and his verbatim repetition of his discussion about what he takes to be an unwarranted contrast between the Mesopotamian mythical and the Hebrew historical view of life (pp. 57–59 and 240–42) may reveal some carelessness in putting his book together.

The purpose of his work, Van Seters tells us, is to search "for the intellectual forms of history in ancient Israel" (p. 354), to discover "what history meant in ancient Israel" (p. 248), "to uncover the first Israelite historian" (p. 7). In order properly to introduce that search, he first reviews in six chapters his presuppositions and method of procedure; then early Greek, Mesopotamian, Hittite, and Egyptian historiographies; and finally texts and inscriptions from the Levant. But he concludes somewhat surprisingly with the accepted wisdom: "for the Egyptians, the Hittites, and the nations of Mesopotamia these historiographic genres did not lead to true history writing" (p. 354).

The four subsequent chapters on Israelite historiography reveal Van Seters creatively at work on material with which he is completely at home. His thesis is that "the first Israelite historian, and the first known historian in Western civilization truly to deserve this designation, was the Deuteronomistic historian" (pp. 356, 362), that is, the author, as Martin Noth first hypothesized in 1948, of the account in the biblical books of Joshua, Judges, Samuel, and Kings. This thesis challenges the usual early dating of the Yahwist, as Van Seters first did in his *Abraham in History and Tradition* (1975), and posits the primacy of Deuteronomy and the Deuteronomistic history in relation to what is thereby deemed to be the subsequent work of the Yahwist (p. 323 and following). To reach this conclusion Van Seters succinctly reviews scholarly opinion about history writing in Israel, asserting that it began in Israel not with epic or legend or etiology, but with an account of contemporary events, the successions of Saul and David to the throne (pp. 219, 346). Van Seters argues his thesis clearly with careful annotation and makes an illuminating contribution to scholarly discussion. Conclusions, bibliography, and three indexes round out the book.

If he occasionally overstates his conclusions or goes beyond his evidence, these lapses do not diminish Van Seters's achievement in forcefully bringing

the study of history writing in ancient Israel within the purview of all students of history.

JOHN H. MARKS
Princeton University

PAUL M. MARTIN. *L'idée de royauté à Rome: De la Rome royale au consensus républicain*. (Miroir des civilisations antiques.) Paris: Adosa. 1982. Pp. xxv, 410. 300 fr.

It is notorious that the expulsion of the kings who had ruled Rome during its first two hundred and fifty years left the Romans with a deep and abiding antipathy for the words *rex* and *regnum*. Yet their second great constitutional revolution restored the monarchy after some five centuries of republican rule, and for the author of this book their behavior indicates a sort of love-hate attitude that requires a careful reexamination of regal Rome to explain. Naturally, Paul M. Martin admits that much of what Livy and Dionysius say about the kings is pure fiction; but he is nevertheless confident that exploitation wherever possible of archaeological evidence, erudite scrutiny of the Romans' Indo-European background in the manner of Dumézil investigating Roman religion, and, not least selective and aprioristic use of traditions and practices that survived into later times will reveal what really happened in Rome of the kings.

Martin agrees with the later Romans that their monarchs were not hereditary; they were, within limits, elective. To succeed him a king had to choose, not his own son, but a male cognate, related to him through a female, a son-in-law, for instance, or a sister's son. By dint of ingenious if somewhat Procrustean reasoning, Martin fits all seven Roman kings into this pattern, four of them, starting with Romulus, the great prototype, Latino-Sabine natives, the remainder Etruscans. The monarchy fell, partly because the Etruscan three tried to make it hereditary, but chiefly because they outraged Roman patrician sentiment by favoring plebeians (for example, they admitted new *gentes* into the senate, began laicizing the law, and so forth). By expelling the kings, the patricians acquired *libertas*, and Camillus and others exploited it to consolidate their class into a caste that controlled the state and did not hesitate to execute recalcitrants like Cassius, Maelius, and Manlius as aspirants to monarchy. It was only when the plebeians conformed to patrician ideology and their leaders began to participate in the great state offices that the republic entered its golden age.

It is improbable that scholars in general will find this account of regal Rome convincing, no matter how interesting they find much of it (for example, Martin's emphasis on the ethnicity of Livy's account of regal wars [p. 173 and following]). Martin himself

provides grounds for scepticism. His repeated insistence that Rome had many Etruscan kings, including several Tarquins, presumably means that the only three recorded are composite figures. How trustworthy then is Martin's surprisingly detailed portrait of Tarquinius Superbus, for him a much maligned hero? But, whatever the reservations, one must admire Martin's encyclopedic knowledge and acute sifting of the ancient evidence. And his final two chapters on the first two centuries of the republic are positively fascinating: even those of us who find his account implausible as a whole will welcome his brilliant demonstration that the year 449 as recorded merely repeats 509, beginning with the names of the consuls (p. 294 and following).

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LAWRENCE KEPPIE. *Colonisation and Veteran Settlement in Italy, 47-14 B.C.* London: The British School at Rome. 1983. Pp. xv, 233. £15.00.

This well-conceived and clearly written monograph adds significantly to our understanding of the period of transition between the late Roman republic and the early principate. Between 47 and 14 B.C., as many as one hundred and fifty thousand veterans of the Roman army received grants of land from Julius Caesar, the triumvirs, or Augustus. In Italy, the mechanism overwhelmingly favored for veteran settlement was colonization. Discharged soldiers received parcels of land, sometimes in new towns created at the moment of settlement (for example, Aosta, Turin, Concordia, Lucus Feroniae), but more normally in already existing townships; the numbers of new settlers ranged from the hundreds (at places like Fanum, Sutrium, and Pisae) to some thousands (Cremona, Bononia, Turin, Placentia). Caesar, his assassins, and his avengers in their turns were forced to hold out special inducements to keep troops loyal, and their soldiers demanded land, with the substantial improvement in social status that might result, as a reward for military terms (that might last as little as six years). Thus, a direct equation between military service and land settlement came to be established, an equation broken finally in 14 B.C. when, with Rome's enemies again non-Romans and peace and prosperity restored to Italy, Augustus effectively removed the army from politics, and substituted cash gratuities in place of grants of land.

In contrast to earlier periods, when motives for colonization were primarily social and economic, and also to later years, when the strategic and defensive aims familiar from the early days of Roman colonization resurfaced, Lawrence Keppie shows that the purpose of veteran settlements in

these years was far more basic: "the removal from the national scene, with all possible speed, of the veterans . . . whatever the disruption and distress that resulted" (p. 128). But if the purposes were simple, the impact of veteran settlement on the towns and countryside of Italy was profound, as Keppie demonstrates in the book's best chapter (pp. 101–32). At the beginning, the friction between the old and new inhabitants of towns is reflected in Latin literature: townsmen hostile toward incoming veterans, with their uncouth ways and lack of agricultural competence; the newly arrived colonists overbearing toward the local populations, many of whom they dispossessed.

But Keppie goes on to make a strong case for the easing of tensions with the passage of time, and for the veterans' eventual integration into their new social surroundings. Tribunes and centurions especially, who were rewarded more handsomely than the rank and file, moved into positions of leadership and were elected to high office in their new home towns, sometimes without losing their sense of earlier identity. Direct and generous intervention by Augustus on his colonies' behalf, which might take the form of public works and other benefactions, could also smooth tensions and facilitate social cohesion between old and new inhabitants. Above all, Keppie persuasively argues against earlier scholars, that veterans, instead of selling their plots and moving off to the city, or functioning as absentee landlords while residing comfortably in country towns, promptly settled down to the serious business of farming; veterans' gravestones in diverse parts of Italy have been discovered well away from the town sites of their respective colonies, which supports the thesis that the men were buried on lands which they owned, worked, and inhabited. The settlement programs also promoted a further mixing of the populations of Italy, and the further weakening of local dialects and cultural differences.

Keppie's study improves on the work of predecessors in making full and skillful use of inscriptional and archaeological evidence, as well as literary. A catalogue, town by town, of sites and settlers (pp. 135–207) forms a valuable complement to the introductory chapters, where the identification and dating of colonies, the military history of the period, and the programs and processes of veterans' settlement are reviewed. A brief epilogue on later settlement in Italy places the triumviral period in broader historical perspective; the numerous lists, maps, plans, and illustrations are welcome and serviceable. Less useful as an aid is the sylloge of veterans' inscriptions that closes the volume: nonspecialists would have been better served (and much wasted space better employed) had the author expanded Latin abbreviations and perhaps offered translations.

Some readers may be discouraged to learn that the ninety surviving inscriptions of individual veterans, which combine to form a large portion of Keppie's evidential foundations, represent less than 0.1 percent of the veterans known to have been settled in this period. We shall continue to wish to know more—about veterans' economic activities outside agriculture, for example, or about their marriages and social ties. But the patient and discriminating labor that has gone into the making of this book is impressive: within its somewhat austere limitations, Keppie's study will remain definitive for years to come.

JOHN H. D'ARMS
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RONALD SYME. *Some Arval Brethren*. New York: Clarendon Press of Oxford University Press. 1980. Pp. vi, 132. \$29.00.

Little is known of the arcane cult of the Arval Brethren, an ancient Roman priestly college originally concerned with agricultural rites. Augustus rescued the *Fratres Arvales* from oblivion, and inscriptional fragments from their *Acta* shed some light on the brotherhood especially for the Flavian period at the end of the first century A.D. Ronald Syme's monograph is a detailed prosopographical analysis of the brethren of Julio-Claudian and Flavian times based on the inscriptions as well as allusions in ancient literary sources.

There were twelve members of the college selected from the senatorial aristocracy (including always the reigning emperor), and they served for life. Vacancies were filled by cooptation, but the influence of the emperor must have been decisive in the selection of new members. Their most important religious ceremony occurred in May in honor of the goddess Dea Dia, and the hymn of the Arval Brethren, which is linguistically perhaps as old as the fifth century B.C., has been preserved in the *Acta*. After the death of Augustus ceremonies in honor of Rome's newest god were incorporated in the functions of the brethren, and from that time they showed special interest in the ruling dynasty.

Syme has subjected the surviving fragmentary lists of members of the college to the kind of microscopic, prosopographical analysis for which he has become justly famous. He shows that the brethren of the Flavian period generally lacked the high distinction of their colleagues in the earlier Julio-Claudian period and says of the Emperor Vespasian and his sons that "it is by no means clear that the rulers attached much importance to the Brethren" (p. 2). But he uses the prosopographical data as the basis for a masterful series of essays about Roman social and political activities. In that sense this mono-

graph should be read as an interesting addendum to his survey of the senatorial aristocracy in his monumental two-volume work, *Tacitus* (1958).

General readers will find some of the chapters interesting and rewarding: "Mortality at Rome," "Kinship and Patronage," "Antiquarian Erudition," and "Ritual and Society." The rest of the monograph will be difficult reading for anyone not familiar with Syme's style and approach. The book desperately needs an introductory chapter on the role and function of the Arval Brethren, but, as it stands, students of the Flavian period will find it a most welcome addition to the literature.

ARTHER FERRILL

University of Washington

JUSTINE DAVIS RANDERS-PEHRSON. *Barbarians and Romans: The Birth Struggle of Europe, A.D. 400–700*. Norman: University of Oklahoma Press. 1983. Pp. xix, 400. \$29.50.

Barbarians and Romans surveys the transition from the late Roman empire to the emergence of medieval kingdoms. One cannot refrain from asking why no professional English-speaking historian since J. B. Bury, writing at the turn of this century, has undertaken a general survey of these troubled times. After gracefully acknowledging her amateur status in the preface, Justine Davis Randers-Pehrson offers a book of broad historical interest that even the professional should appreciate for its wit and candid assessments of our scholarly nitpicking. The story begins at Celtic Trier and ends in Bede's England. Included are visits to Saint Ambrose in Milan, the emperors in Constantinople and Ravenna, and the barbarian kings of the Goths, Vandals, Franks, and Anglo-Saxons. Not even the remote caves of the Egyptian desert or the monastic isles off the coast of Ireland are overlooked.

The author brings to her work extraordinary personal intimacy with the geography and artistic splendors of the era. She is also remarkably at home in the turbulent academic crossfires that characterize this period of study. Her writing style hardly compares to the stately prose of Gibbon and Bury, of whom she is so fond. Yet despite the frequent colloquialisms that punctuate her newsmagazine-like style, the book carries the reader along swiftly and vigorously conveys the pathos, desperation, and ecstasies of these centuries. The author is at her best writing biography: her depictions of Constantius III, Galla Placidia, and Wilfrid are especially sensitive. The true merits of the book are its breadth of vision, common sense, and obvious love of the people she describes.

There are weaknesses, however. The bibliography is seriously dated, overlooking important works

in many areas. For example, there is no mention of the "Reihengräber civilization" among the early Franks and Alamans (see further H. Böhme, Lucien Musset, and Edward James). Indeed, there is very little meaningful discussion of the lower classes that have only begun to emerge more clearly during the last decade from the sparse archaeological records (see further Max Martin, Volker Bierbrauer, Herwig Wolfram, and Malcolm Todd). Nonetheless, the book's strengths clearly outweigh its shortcomings. *Barbarians and Romans* bridges a widening gap between semipopular history and the works of the professional heirs of Ranke and Mommsen. Students and their mentors will find this survey provocative and informative.

THOMAS S. BURNS

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KENNETH G. HOLUM. *Theodosian Empresses: Women and Imperial Dominion in Late Antiquity*. (Transformation of the Classical Heritage, number 3.) Berkeley and Los Angeles: University of California Press. 1982. Pp. xiv, 258. \$25.00.

This book is much more than a collection of imperial biographies. Rather, it is an analytic study of the nature of imperial rule and the way it was exercised by the women of the Theodosian dynasty (A.D. 379–454). Kenneth G. Holum's primary thesis is that the role of *basileia* underwent striking changes between the reign of Flacilla, wife of Theodosius I, and that of Pulcheria, sister of Theodosius II. At the beginning of this period the empress was primarily a childbearer, and she was granted important new prerogatives because of her significance for the continuity of the dynasty. In the early years of the fifth century, however, Pulcheria transformed this idea, in part by her vow of virginity and her association with the cult of the Virgin. Under Pulcheria the position of *basileia* took on a sacral character of its own, which balanced the military and magistral aspects that were confined to her male counterparts.

Holum has written a stimulating and generally well-reasoned book that makes an important contribution to our understanding of early Byzantine political practice and theory. He has shown (although he makes little of the point) that the powerful Byzantine empress one sees dimly revealed in the pages of Procopius and the *Book of Ceremonies* developed largely in the years of the early fifth century.

There are naturally many things that one finds missing in this book. Holum, for example, says almost nothing about changes in the position of women in society as a whole at this time: surely these had some effect on the position of the empress.

Further, some of the author's arguments seem rather to assume too much: for example, that practically all of Theodosius I's actions were motivated by dynastic concerns and that Pulcheria lay behind almost all important developments, even during her period of weakness. Certainly, not all readers will be convinced about the reality of "Pulcheria's crusade." But this is one of the almost inescapable difficulties with a book of this nature: in order to maintain the theme the author must occasionally overemphasize the importance of his subject. In the present instance Holum's arguments may not always be totally convincing, but the work as a whole is of obvious value. Particularly to be praised is his integration of artistic and numismatic material and his sensitivity to the care with which ideological programs were expressed by the imperial mints and workshops.

TIMOTHY E. GREGORY
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MEDIEVAL

WENDY DAVIES. *Wales in the Early Middle Ages*. (Studies in the Early History of Britain.) Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. xii, 263. \$46.25.

This is a brave book. Not since Sir John Lloyd's classic study of Wales prior to the death of its last independent prince in 1282 has an attempt been made to survey the history of Wales between the Roman withdrawal by the beginning of the fifth century and the Norman incursions toward the end of the eleventh. The quantity of written evidence has not significantly increased since then, but the technical and intellectual problems facing the interpreter of medieval Welsh law, saints' lives, charters, genealogies, annals and place-names—usually recorded in Norman or later manuscripts—have become all too apparent. The revolution in archaeology, however, has enabled the Welsh Archaeological Trusts to provide Wendy Davies with some of her more exciting nuggets of information; it remains to be seen whether the archaeologists' chronology is sufficiently certain to support all the conclusions now being drawn from site excavations and chance finds.

This is also a modest book. Davies is aware that she is providing an interim survey of a fast-changing scene. Her description of it as an "exercise in speculative imagination, [rather] than a sober, well-documented analysis" (p. 1) does justice to her intelligent posing of fundamental questions, but it undervalues the range of sources she uses and her skill and restraint in interpreting them. The book falls into three well-organized parts. Three chapters

describe the "face" of Wales and its economic exploitation and social structure. These represent the most significant departure from Lloyd's account. Two chapters deal with the morcellated political structure of pre-Norman Wales—the powers of kings and the community's role in enforcing law and order. Two chapters are devoted to the church—an "episcopally dominated church, though a church largely consisting of monasteries" (p. 164)—and to Welshmen's devotion to a faith whose cults gave it a localized dimension. Full annotation, a detailed bibliography, and numerous maps, plans, and photographs complete a well-produced volume.

Davies gleans precious evidence wherever she can find it in these five and a half centuries. This paucity of evidence runs a danger of constructing a model for early medieval Wales that submerges the dynamic element even in a relatively primitive society. Davies thereby invites other approaches to the study of early medieval Wales that may prove less schematized and static than her own. Nevertheless, she does note certain major changes, notably from the ninth century onward: population recovery, the growing numbers of poor, the stability and expanding powers of kingdoms, and the emergence of both an Anglo-Welsh frontier and a Welsh cultural identity. Davies's heavy reliance on her earlier work on the charters of the bishopric of Llandaff (as recorded later in *Liber Landavensis*) raises the question as to how far the experience of the southeast was shared by the rest of Wales, where comparable material does not exist. As Davies suggests, important differences persisted until shortly before the Normans arrived. Indeed, the nature of the surviving sources is so fundamental a theme that to relegate its discussion to an appendix is unfortunate. Missing, too, is a comprehensive comparison with contemporary English and Continental society, although there are valuable hints in relation to the church.

This is one of the most stimulating books to have been written on medieval Wales for decades. One admires its command of sources, its astute speculations, and its lucid exposition. If its suggestions and conclusions merit testing at almost every turn, this is because the book is the best companion, guide, and informant for those following in Davies's footsteps. It puts at the disposal of medieval scholars in general a thoughtful, up-to-date statement on early Welsh history comparable with those available on England, France, and Italy.

RALPH A. GRIFFITHS
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SANDRA RABAN. *Mortmain Legislation and the English Church, 1279–1500*. (Cambridge Studies in Medieval Life and Thought, third series, number 17.) New

York: Cambridge University Press. 1982. Pp. xiii, 216. \$39.50.

Sandra Raban's carefully researched monograph illustrates vividly the frustrations and rewards of medieval historical research; frustrations, because the basic questions about the statute of mortmain (Why was it introduced? and Did it affect the amount of property accumulated by the church?) cannot, despite diligent inquiry, be answered with certainty; rewards, because investigation of this one statute leads to discussion of such larger themes as economic development and changing lay piety in late medieval England. The statute *De viris religiosus* followed nearly a century of concern about the church's acquisition of land and anxiety about lords' rights when real property passed permanently into corporate ownership. Political and military considerations as well as legal confusion help to account for the framing of legislation, yet not even the author can say why the statute was passed in 1279 or why its terms were so severe.

But she can tell us how the statute worked: how licenses to circumvent it were obtained; what the different types of license were; and what costs were involved. She scotches speculation that financial motives lay behind the measure by showing that it was not until 1299 that the crown began using mortmain licenses as a source of revenue. Enforcement was usually the responsibility of the escheators, who were vigilant, thorough, and occasionally harsh. Yet infringements there certainly were, and Raban provides a blueprint for successful evasion. A chapter is devoted to clerical exploitation of legal loopholes, which included fictitious property disputes, leasing, and enfeoffments to use—the last forbidden by legislation in 1391.

Did mortmain legislation prevent the acquisition of property by the church? Although unable to give a firm answer, the author makes some pertinent comments. The incidence of land donation was already much reduced by 1279, but land purchase, especially by wealthier monasteries, was brisk. This seems to have been sharply checked by the statute. Although licenses were never refused, some alienations may have been prevented by the additional costs and delays imposed by the law. It seems likely, however, that economic conditions and changing fashions of pious charity were more effective barriers to ecclesiastical acquisitions than was legislation.

The author has drawn her material largely from Lincolnshire and the Fens. Accounts using other regional bases might modify her conclusions in detail though surely not in general since the problems of sources, their survival and interpretation, are not peculiar to one area. In conclusion, two general strengths of this work should be mentioned. One is its emphasis on constant change in the

operation of the law to 1500. The other is the multifaceted nature of the story itself, which will make the book valuable to scholars in several fields.

A. K. MCHARDY
University of Aberdeen

ALLEN J. FRANTZEN. *The Literature of Penance in Anglo-Saxon England*. New Brunswick, N.J.: Rutgers University Press. 1983. Pp. xiv, 238. \$27.50.

The sins of a society and the system by which the sinner is reconciled to its ideals illuminate the entire societal world and not only its darker side. Allen J. Frantzen's brilliant study of the penitential or handbook of penance proves this. Penitentials indeed present "an image of early medieval society" in its unity and diversity—from property to sex, from law to poetry, from holiness to authority. In this book Frantzen traces the history of Irish, Anglo-Saxon, and Frankish penitential handbooks. Although he has "chosen to write about the sources themselves, the textual and contextual problems which they raise, and their relevance to the literature of Anglo-Saxon England" (p. x), he also raises questions (and provides answers) crucial for understanding medieval life.

Frantzen's opening chapter is a superb introduction to both the subject and his approach. He attacks the superficial but fashionable treatment of penitentials as a means of social control and demonstrates that they "involve attitudes toward the medieval church and even Christianity itself." Frantzen identifies a break with public ritual penance as early as the sixth century in Ireland. The increased practice of private confession and performance of penance led to the development of guidebooks for priests. Whatever their relationship to pre-Christian and non-Irish sources, these guidebooks, or penitentials, were a product of Irish monasticism and spread by the Irish missionary movement. Frantzen effectively shows that penitentials are part of that Irish culture that is seen as "distinctive without being bizarre, original without being unorthodox, and traditional without being commonplace" (p. 59).

"Penance and Prayer in Eighth-Century England" (chap. 3) studies English adaptation of these Irish influences on levels of both administration and individual "vocabulary for self-expression." English handbooks are analyzed at length, as is the relationship of penance to secular law, pastoral activity, and literary records. Greater changes occurred when penitentials reached the Continent: both theory and use were systematized and the handbooks were integrated into Continental traditions (chap. 4). The dichotomy between public and private penance resulted in more formally standardized, larger handbooks, and the impact of confessional prayer on

public rites. Tenth-century England witnessed a synthesis of Frankish reforms with insular tradition. The simplification that resulted from this synthesis recaptured the penitential's original instructional function (chap. 5). One of the most valuable of Frantzen's explorations is the impact on Old English literature (chaps. 6–7). His emphasis on Aelfric's *Catholic Homilies*, "a monument which dwarfs all else written about penance in his period," does not diminish the heterogeneity of other materials the author consults relating the theme of penance to both form and content of Old English poetry.

A stimulating epilogue, "A Look Ahead to New Questions," asserts not only the need for new textual editions and closer study of the early Irish handbooks but also the relevance of the topic for social history and late medieval studies. Here Frantzen again answers modern critiques of penitential literature. An inclusive twenty-page bibliography and a careful index conclude this rich and scholarly work. It would be extremely difficult for anyone, after reading Frantzen's illuminating book, to agree with Charles Plummer that "it is hard to see how anyone could busy himself with such literature and not be the worse for it."

WILLIAM A. CHANEY
Lawrence University

CHARLES-M. DE LA RONCIÈRE. *Prix et salaires à Florence au XIV^e siècle, 1280–1380*. (Collection de l'École Française de Rome, number 59.) Rome: École Française de Rome. 1982. Pp. x, 867.

This book contains the first two of three parts of the author's *thèse d'état*, originally published in typescript in 1975. Its publication in book form is a welcome event. Charles-M. de la Roncière's erudite and meticulous scholarship, displayed in important published articles, is now available in a more comprehensive form to a wider audience. The discussions of the sources and the methods used to exploit them alone provide fascinating insights into the historian's craft from a skilled practitioner. It is what de la Roncière does with his sources, however, that will make his book a considerable presence in future research.

Book 1 tracks the movement of prices and wages from 1320 to 1380, a period of economic and demographic disasters that ends in political turmoil. De la Roncière charts the prices of wheat and other cereals, wine, meats, beans and vegetables, and craft products. He then does the same for wages of master masons, construction workers, agricultural laborers, and textile workers. In the process there emerges a revealing depiction of shifting patterns of consumption and changing conditions of employment.

De la Roncière finds a general continual increase in prices, varying by commodity. Prices fluctuated seasonally and stabilized in 1353–68. Salaries also rose, varying with trade and fluctuating with conjunctures of economic activity. Using a speculative typical budget and nutritional information, the author determines that at critical moments life must have been difficult even for relatively highly paid workers, but he also concludes that after 1348 higher wages led to a changing consumption pattern that mirrored that of the Florentine patriciate.

In book 2 de la Roncière analyzes the causal factors in the formation of prices and wages in Florence. Examination of the complicated monetary situation reveals that the stable gold coin, the florin, was the real standard of value and that debasements of silver coinage had little effect. He finds that Florence's sophisticated grain policy did tend to stabilize prices, but within limits. A more important factor was population. One of the author's most significant findings is that post-plague migration from the countryside to the city greatly altered the economic balance between the two, leaving the *contado* relatively impoverished and deprived of its most productive inhabitants. De la Roncière sees an inverse relation between population on the one hand and wages and prices on the other, although he does hasten to add that population was still only one factor. Climate also had a great impact, whereas war had little influence on prices. Finally, in comparison with the rest of Europe, Florence's economy, says the author, recovered quickly from the disasters of the 1340s, although its population did not, hence wages remained high longer.

All of de la Roncière's economic and demographic findings are brought to bear on the final, brief discussion of the Ciompi revolt of 1378. The author sides with those who have refused to see the Ciompi as a revolutionary proletariat, concluding that the revolt occurred as long- and short-term developments worsened conditions for the workers, whose demands remained largely traditional. Although this is already a big book, the discussion of the role of the new ideology of poverty in the events of 1378 might have been expanded and more thoroughly integrated with the previous analyses. Ideological dimensions of economic activity do not otherwise figure in the book.

Such a brief review cannot begin to expose all the important and fascinating aspects of this book. Economic historians will want to pick carefully through its crammed pages. Those who study the ideological aspect of economic actions will want to expand on or contest the book's conclusions. But it is safe to predict that a generation of scholars will now have to confront de la Roncière's solid contribution.

THOMAS KUEHN
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JÜRGEN HANNIG. *Consensus fidelium: Frühfeudale Interpretationen des Verhältnisses von Königtum und Adel am Beispiel des Frankenreiches*. (Monographien zur Geschichte des Mittelalters, number 27.) Stuttgart: Anton Hiersemann. 1982. Pp. viii, 343. DM 198.

Jürgen Hannig set out to study the *consensus/consilium fidelium* formulas in Merovingian and Carolingian texts in order to clarify the legal relationships between kings and the nobility. He has produced a fascinating study that makes valuable and highly original contributions not only to legal history but also, more importantly, to political, social, and ideological history. Actually, he demonstrates the dangers implicit in studying any one of these kinds of history in isolation, while hammering another nail in the coffin of nineteenth-century constitutionalist views of early medieval developments.

A bit of background may help clarify Hannig's achievement. From Fustel de Coulanges to F. L. Ganshof, French scholarship has been content to regard Frankish kings as despots. German scholars have seen kings as limited either by the *Volk* as a whole or else by some sort of nobility. Where nobles have been seen as a brake on monarchy their "rights" have been interpreted either as a result of their place in a *Gefolgschaft* or as a consequence of their having been somehow representatives of the *Volk*. For the French, therefore, *consensus* simply did not exist even if the texts seem to imply that it did, whereas for the Germans *consensus* was real. The problem has been to figure out how it worked in practice.

By a careful analysis of Merovingian and early Carolingian sources of many kinds (capitularies, conciliar *acta*, diplomas, hagiographies, narratives, annals, and so on), Hannig shows that *consensus* was neither required nor guaranteed as a matter of law, custom, or convention. No noble *Mitspracherecht* can be found in any of these sources.

In the Carolingian capitularies and in other kinds of sources from the early ninth century, *consensus/consilium* formulas are ubiquitous. Hannig explains this apparent change brilliantly and persuasively by showing that it was rooted in certain kinds of Merovingian episcopal activity, gained currency amid seventh-century political tribulations, progressed in the time of Boniface, and then developed further during the reigns of Charlemagne and Louis the Pious. What happened was neither the gradual assertion of a noble *Mitspracherecht* nor the slow reappearance of primitive Germanic folk democracy but rather the articulation of a new Christian ethic of rulership coupled with a need by the Carolingian usurpers to find a new basis for governmental legitimacy. Kingship came to be defined as a ministry for which the king was answerable to God. In order to learn how best to govern, the king was

supposed to listen to God's other ministers, the clergy. The nobility was gradually absorbed into this ideological matrix in such a way that they had to give their advice in a common effort to discern the will of God. This set of ideas reached fruition just as the Carolingians assumed the royal office and began broadcasting an effective propaganda aimed at proving that the nobility had consented all along to their assumption of power and to the implementation of their governmental program. In other words, it was a Christian theology of government and a set of concrete political circumstances that conferred on the nobility its right to advise and consent. Primitive *Germanentum* had nothing at all to do with the whole business. By the time of Louis the Pious the nobility had learned to advise—whether requested to or not—and, with disturbing regularity, to withhold consent.

I wish Hannig had dealt a little more fully with political realities (as Karl Brunner did in a recent book). I do not think Hannig has explained away all of the importance of the political dynamics of the *Gefolgschaft* model of royal-noble interaction. I also wonder how Hannig would respond to this question. Are we to assume that Germanic nobles—whatever we suppose "noble" to mean—had no rights vis-à-vis kings until clerical theoreticians and the complexities of Carolingian politics suggested to them that they did? Some publications escaped Hannig's attention—Patrick Wormald's crucial article on the Germanic law codes as well as studies by Walter Ullmann, Karl Morrison, and Janet Nelson. The book is awash in printer's errors too. Still, these are minor criticisms. This is a masterful book that cannot be overlooked by any serious student of the early Middle Ages. I predict that it will soon stand at the center of a spirited and fruitful debate about early medieval law and government.

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FRANTIŠEK GRAUS. *Die Nationenbildung der Westslawen im Mittelalter*. (Nationes: Historische und Philologische Untersuchungen zur Entstehung der Europäischen Nationen im Mittelalter, number 3.) Sigmaringen: Thorbecke. 1980. Pp. 260. DM 84.

This book will interest both specialists in medieval East Central European history and those who deal with the question of nationalism. After a brief theoretical chapter and a treatment of the "pre-history" of nation formation in this region, František Graus, the distinguished Czech medievalist now living and teaching in Basel, moves to a systematic study of Moravia, Bohemia, Poland, and the Polabian Slavs in the Middle Ages. Only in Bohemia and

Poland, according to Graus, did national consciousness fully develop, and it is his treatment of their histories that forms the core of this book.

Graus does not impose a rigid developmental schema on the phenomena he describes. Rather, it is his intent to identify the different manifestations of his subject. Thus he notes the emergence of an early *Wir-Gefühl*; sees evidence of eventual dynastic consciousness; and delineates several forms of territorialized consciousness: simple *Heimatgefühl*, *Landespatritismus*, and abstract consciousness of the *regnum* or the *corona regni*. Full national awareness, however, was only achieved after the thirteenth century. The clergy in particular was able to formulate and articulate this consciousness. Eventually, townsmen also became involved, but only after Slavonic urban populations emerged to struggle with the predominantly German townsmen who had arrived as part of the great process of colonization in the high Middle Ages. (Indeed, Graus identifies this wave of settlement as one of the most crucial elements in the emergence of national consciousness.) Finally, in phenomena such as the Hussite movement, the full nation became conscious of itself. Bohemia is used throughout as the paradigm, for Graus argues that Poland's progress toward the goal of full consciousness was retarded by political fragmentation.

This is a very subtly argued work, made more so by the eighty pages of *Beilagen*, which provide close analyses of the sources and of crucial terms, and by extensive notes and bibliography. At one level the book succeeds brilliantly. It provides a comparative political and institutional analysis of Polish and Bohemian history and ideas. In addition, the author is able to draw on his own rich and sophisticated understanding of the concept of the nation in the Hussite period to throw light on other developments. But there are some problems that remain. In a comparative treatment, particularly one that argues that German colonization marked a major caesura in the history of the whole region, the absence of a treatment of Hungarian affairs raises questions as to whether the author's generalizations are really as useful as they appear at first glance to be. More fundamental, however, are some conceptual problems. In Graus's treatment, the subject of the book's title, "the formation of nations," tends to become equated with national consciousness, which in turn is often used interchangeably with nationalism. At times one must ask what phenomenon is under discussion. Moreover, the very concepts of nationalism and national consciousness are notoriously elusive. Not all who read this book will be fully convinced that Graus's impressive source study and analysis of consciousness is matched by a workable conceptual structure.

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BERNARD SEPTIMUS. *Hispano-Jewish Culture in Transition: The Career and Controversies of Ramah*. (Harvard Judaic Monographs, number 4.) Cambridge: Harvard University Press. 1982. Pp. ix. 180. \$20.00.

One of the most important aspects of Jewish cultural development in medieval Spain was the Maimonidean controversy—the long and bitter controversy among rabbis over the novel and possibly heretical views expressed by Maimonides concerning resurrection, immortality, and the afterlife. Meir Abulafia, chief rabbi of Castille later in his career (a fact nowhere mentioned in the book under review), began this controversy in Spain. Bernard Septimus's book, apparently a doctoral dissertation, examines Abulafia's role in this controversy and tangentially deals with some other aspects of his thought. In a recent article (*Sefarad*, 41 [1981]) I had occasion to remark that the subject of the Maimonidean controversy was not one for treatment by amateur historians. This book is certainly a step in the right direction and is a considerable improvement over earlier books on the subject. Nevertheless, it is not without faults. One is constantly annoyed by the failure to conform to any acceptable standard of scholarly citation. There is no bibliography at all. Nowhere is there a systematic list of editions and manuscripts of Abulafia's work. References in footnotes are very casual, with initials often used instead of full names, lack of full bibliographical data, and so on. The author apparently assumed that only Jewish scholars would read his work, and so no identification is provided for the numerous names of Jewish figures (Judah Ibn Ezra, Joseph Ibn Megash, and so forth). Some names are incorrectly spelled (for example, Ibn Shushan, not "Shoshan," Ibn al-Fakhkhar, and so forth). Although it appears that the author has done a thorough job of research, closer analysis reveals that he hardly utilized the extremely important published *responsa* of Abulafia, did not consult at all additional *responsa* in manuscript, and failed to examine some very important secondary material.

The chief section of interest is chapter 3, which deals with the actual controversy. This is the heart of the book and could stand alone without the additional chapters, and as such would have made a significant contribution. Even here there are problems. The most serious objection is that Septimus has paid no attention to the repeated suggestion of earlier scholars that Abulafia's views are related to those of an earlier controversy between Samuel b. ʿAli, gaon of Baghdad, and Maimonides himself. Indeed, Septimus does not even mention Samuel.

There are other problems that space does not permit to be discussed here. Nevertheless, the book provides many insights and is a helpful contribution

to our knowledge of an extremely important issue in Jewish culture.

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MODERN EUROPE

RETHA M. WARNICKE. *Women of the English Renaissance and Reformation*. (Contributions in Women's Studies, number 38.) Westport, Conn.: Greenwood. 1983. Pp. viii, 228. \$29.95.

As number 38 in Greenwood Press's distinguished series, "Contributions in Women's Studies," Retha M. Warnicke's *Women of the English Renaissance and Reformation* clearly outclasses other monographs of its type in the field. For example, it is more analytical and more ambitious in aim than Pearl Hogrefe's *Tudor Women: Commoners and Queens* (1975). Warnicke's goal is to identify and describe the contributions of educated women and religious activists through a generational study from the pre-Reformation through the Jacobean eras. An underlying theme, "the English acceptance of classical training for women" (p. 4), surfaces now and again throughout the study of the four generations.

The work begins with an analysis of attitudes toward women as expressed in More's *Utopia* and moves to a description of the educational method fostered in the More household (particularly emphasizing the education of More's daughter Margaret). This is followed by discussion of the accomplishments of women who had reputations for learning, such as Catherine of Aragon, Anne Boleyn, and Lady Jane Grey, and pedagogical humanist works. Disturbing the continuity of her discussion of humanist learning, the author then reviews the dynastic struggle from 1525 to 1587 that brought two women to the throne and analyzes some of the literature that criticized their rule. In "Religious Persecution, 1533-1558," she returns to a discussion of the persecution of Protestant and Catholic religious activists and closes the chapter with what seems a digression—a discussion of both Protestant and Catholic attitudes toward marriage and the family. In "The Reformation Generation" Warnicke evaluates the influence of Henry VIII and the role of his last queen, Catherine Parr, in the education of the royal children.

By the mid-Elizabethan era, humanist educational goals were almost completely shelved in favor of a more practical training for aristocratic girls and women. In "Elizabethan Reformers" the author returns to an earlier theme: the impact of Protestantism on the lives of women and their changed

roles within the church. Again she contrasts Protestants' and Catholics' attitudes toward marriage and family life. According to Warnicke, married Catholic women, far from being discouraged by the recusancy laws, continued to pursue their private devotions, support Jesuit priests, send their daughters to convents abroad, and arrange marriages with other Catholic families.

By the Jacobean era, the impetus for humanistic education had been all but exhausted. Only a small number of Catholic and Protestant women continued the More tradition.

The author cannot be faulted for her exhaustive research, but her style is almost encyclopedic and often mechanical. The book should have been more carefully edited, for repetitious and irrelevant material abounds and there are occasional proofreading oversights.

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ALAN BRAY. *Homosexuality in Renaissance England*. London: Gay Men's Press; distributed by Flatiron Book Distributors, New York. 1982. Pp. 149. Cloth \$15.00, paper \$5.95.

The strengths of this short book—good sense, thorough research, and historical acuity—do its author credit; its weaknesses—scanty sources, arguable interpretations, and conceptual confusion—probably reflect more on the field of sexual history than on the writer. Minor reservations aside, Alan Bray has achieved much.

Chapter 1, "Word and Symbol," outlines English attitudes toward homosexuality in literary and other public contexts of the sixteenth and seventeenth centuries. Bray argues that "sodomites" were feared and despised. They were associated with specific forces of evil (for example, papists) and taken to be representatives of a somewhat ill-defined "disorder" (particularly troubling to Elizabethans with a passion for "orderliness"). Yet this does not seem to be an adequate explanation for the visceral hostility Bray posits, especially when the evidence is so formulaic. Why, for example, are literary praises of Ganymede dismissed as *topoi* while literary condemnations are taken at face value? Despite some subtle points, this chapter is ultimately the weakest in the book.

Chapter 2, "The Social Setting," introduces the reader to the sources available to the historian of homosexuality (pp. 33-42) and to social factors bearing on the place of homosexual behavior in English society from 1550 to about 1650. Bray's comments on the limitations of the sources will seem jejune and unnecessary to professional historians,

but his observations about the constraints affecting all sexual behavior during the period are incisive and valuable. For example, the number and place of servants in English households at the time must have had enormous impact on the variety and nature of sexual interactions, and Bray's treatment brings this into sharp relief.

Chapter 3, "Society and the Individual," addresses roughly the same period, but from a more personal standpoint. Bray asks: How did the individuals involved in homosexual behavior feel about their participation in acts so inimical to the values of their society? His suggestions about how people deceive themselves and others—how they rationalize, justify, equivocate, or openly defy the conventions of their society—are all sensitive, apposite, grounded in a thorough understanding of the historical context, and, ultimately, persuasive.

The fourth and best chapter, "Molly," describes the homosexual world of eighteenth-century London centered in Molly houses (eighteenth-century institutions comparable in some ways to gay bars in modern industrialized nations). The Molly houses and their customers were subject to periodic bursts of public outrage that resulted in arrests, trials, executions, and the ruin of many lives. Bray evokes the fear, offended propriety, and tragic consequences of these episodes with haunting clarity and pathos.

But the author does not escape the conceptual muddles currently afflicting historians of sexuality. Following the approach popularized by Jeffrey Weeks, he begins with a confused statement of the "social constructionist" position on homosexuality, which he must recant at the end. Although he makes a conscientious effort to circumvent the problem, Bray is forced both by the sources and the ambiguities of human experience to use terms like "homosexuality," "homosexual behavior," "sodomy," and so forth, in such diffuse ways that statements about when "homosexuality" did or did not exist have little meaning.

If he does not provide answers, Bray does bring such questions into clearer focus. While sketching a startlingly lively picture of the subject, he offers a surer footing for future researchers—not a mean achievement for a volume of less than one hundred and fifty pages.

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ALISTAIR FOX. *Thomas More: History and Providence*. New Haven: Yale University Press. 1982. Pp. xi, 271. \$19.95.

This subtle and closely reasoned book will probably dominate interpretations of Thomas More for some

time to come, even among those who disagree with its major thesis. Alistair Fox is concerned to unravel what have often been called the paradoxes of More's life and thought. They include, for example, the tension between asceticism and humanism and between irenicism and dogmatism. It is the strength of the book that Fox subsumes these particular paradoxes into the larger pattern of More's life.

Some of More's earliest writings, as well as what is known about his life from other sources, reveal a pleasure-loving, sometimes bawdy-minded individual who took spontaneous delight in the world around him. Yet this same young man also lived for a time in a Carthusian monastery and thought seriously about becoming a monk. Eventually, however, he decided against it and instead entered public life. Here he was able to develop his skills as a humanist writer and, seemingly, to pursue political action as a way of doing God's will in the world. As the Protestant Reformation began to have an impact in England, More appeared to turn his back on his earlier humanist principles. Opposing all reform of the church, he wrote dogmatic and sometimes bitter defenses of medieval Christianity. Only during his imprisonment, when he turned his attention to perennial religious themes like Christ's passion, did he recover his earlier balance and profundity. Such is the pattern that has been identified by many authors, yet interpreted in widely varying ways.

Fox proposes an essentially psychological understanding of More's career. During the early part of his life, More was torn by two conflicting impulses—a natural, almost epicurean humanism and medieval asceticism. Guided, probably, by John Colet, More resolved the antagonism of these two impulses through a concept of divine providence according to which man, in acting in the world, could serve as the instrument of God's unfolding will. Thus it was possible to be a certain kind of worldly while remaining a dutiful Christian.

The incursion of heresy, however, upset More's hard-won resolution of this problem by making it seem as if everything he had lived for would be defeated. Thus his fundamental sense of divine providence was called into question. In Fox's view, More had to fight against William Tyndale and other heretics with unusual ferocity because his sanity was at stake, in the form of his own view of the world and his place in it. After first staking his vindication on heresy's defeat, More switched to an apocalyptic mood as that defeat seemed less and less likely. Finally, however, during the Tower imprisonment he was once again able to recover a serene understanding of providence by applying that concept to himself: he would be vindicated through his own death, an act of self-sacrifice that would prove the integrity of his position.

This, in bare schema, is Fox's very complex argument. There is no doubt that he has studied More with great care and that his reading does help to illumine aspects of More's life that have remained puzzling. In the end, however, it seems necessary to ask whether this interpretation is simply too subtle, discarding familiar understandings of More that do at least as much justice to what Fox is trying to explain.

It must be pointed out that the tension between spontaneous worldliness and asceticism was hardly unique in the history of Christianity. Renaissance humanists tried various ways of resolving that tension, and More's solution—a public life of great rectitude along with a private spirituality of considerable rigor—could even be called medieval.

Fox's interpretation also assumes that More had a defective understanding of providence. It has always been fundamental to the Christian understanding of that doctrine that human beings cannot really comprehend its workings nor the ways in which they might be its instruments. As most other commentators have pointed out, More's main doubt was whether he could really accomplish good at the court of Henry VIII or at what point he would be forced to compromise himself. It is also important to note that the "reactionary" More of the religious polemics was employing a style not unknown in theological controversy. His fevered stance of the early 1530s seems to present the familiar sight of the moderate reformer who comes to fear that the dynamic of change will lead to destruction and revolution.

Fox admits that More never really did change his opinions in the Tower. He did speak less harshly of the Protestants, but this was mainly because of the Turkish threat. Although his enforced leisure probably did cause More to lose some of his sense of urgency over the Protestant threat, a more irenic appeal, calling attention to the way in which the Turks menaced all Western Christians indiscriminately, perhaps also seemed to be a promising way of getting the Protestants to reconsider their break with Rome.

Finally, Fox posits More's death as a quasi-mystical leap into the dark. Here the author overlooks not only More's own claim that he simply wanted to be left alone, but also More's use of legal arguments to justify his silence and the fact that a genuinely irenic stand would seem to have dictated an attempt at reconciliation with Henry VIII. More's acceptance of martyrdom would, in classical Christian theology, indeed be seen as the climax of divine providence in his life, but not necessarily for the somewhat elusive reasons that Fox proposes.

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D. M. PALLISER. *The Age of Elizabeth: England under the Later Tudors, 1547–1603*. (Social and Economic History of England.) New York: Longman. 1983. Pp. xix, 450. Cloth \$30.00, paper \$13.95.

There can be few more trying tasks for the historian than writing a book such as this, a single volume in a general social and economic history of England. The very terms—"social" and "economic" history—are by no means self-defining; they trail off into fuzzy uncertainties. And such a book, dealing with an early modern period, faces the additional difficulties posed by fragmentary sources, usually not susceptible to easy interpretation. The period set for the book is dictated by political events, which have no necessary connection with social or economic phenomena. Scholarly explorations of different topics are very uneven and often marked by deep disagreement. Having stated all these obstacles, one should hasten to say that D. M. Palliser has attacked his formidable task with sober good sense and intelligent and fair-minded analysis and has consequently provided a book well above the standard for surveys of this type. It will be of great use to students seeking an introduction to the problems it deals with. It is also a succinct progress report on the state of the art.

The range of topics dealt with is a fairly traditional one. After a brief introduction to the political history of the age, there are successive chapters on demography, the distribution of wealth, and the great inflation. Other sections deal with the more general problems of agriculture, manufacture, urban life, and commerce. The final three chapters broaden out into the more general questions of government, religion, and culture. The later sixteenth century presents particularly awkward choices for the economic and, to some extent, the social historian. The sources are voluminous enough to be susceptible to some quantitative treatment, but they rarely provide a strong base for far-reaching or general propositions. The discrete, impressionistic evidence, literary, anecdotal, or epistolary, is rich but often doubtfully representative. Palliser has steered a careful middle course with considerable success. The volume of statistical material is large enough to provide bulk, but it is set off by a skillful deployment of impressionistic witness. The result is a dense text, packed with information but not so overweighted with it as to stifle the reader.

The quality of treatment in the various areas not surprisingly reflects the extent and depth of current scholarly work. For instance, the account of the historical demography of this age is thorough, nuanced, and reasonably conclusive. So is the lucid chapter on inflation. Perhaps the least satisfactory

sections are those dealing with the more airy topics of religion and culture. These are less tightly argued and necessarily less fully delineated. One might raise the question whether the general assumption of the Longman series—the linkage of “social” and “economic” history—is entirely well grounded.

The roughly half-century with which this book deals is not easily susceptible to generalization. Change there certainly was—of many kinds—but it is not at all evident what the direction of these changes was. Was it an age of urban growth, stagnation, or retrogression? There is evidence pointing in each direction. How far was there real change as against mere quantitative accumulation? It is not possible to give clear-cut and decisive answers to such questions. Palliser has not evaded these difficulties; he has been honest in laying out the alternative answers and modestly self-confident in expressing his own preferred judgments where there is no consensus among scholars. In short, this is a book that does justice to an untidy and somewhat unmanageable subject, offering the audience as informed and judicious guidance as the tangled subject matter allows.

WALLACE T. MACCAFFREY
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A. L. ROWSE. *Eminent Elizabethans*. Athens: University of Georgia Press. 1983. Pp. x, 199.

CAROLLY ERICKSON. *The First Elizabeth*. New York: Summit Books. 1983. Pp. 447. \$19.95.

Seriously flawed and of uneven quality, these two books on Tudor England offer little to the specialist and tend to romanticize history. Carolly Erickson's *The First Elizabeth*, presents an unsympathetic and episodic study of the last Tudor. A. L. Rowse's *Eminent Elizabethans* provides five biographical portraits.

Although Rowse considers Strachey's *Eminent Victorians* as the model for *Eminent Elizabethans*, the book is more similar in plan (if not in quality) to Notestein's *Four Worthies*. Rowse's work is most uneven, leading one to suspect that the book was written over a span of several years. Four of the five biographical sketches are executed well enough, but the study of Bess of Hardwick, countess of Shrewsbury, is blemished by touches of romanticism and the interjection of Rowse's opinions on twentieth-century ills. Noting Bess's philanthropy, Rowse concludes that these gifts “were made possible because . . . [her wealth] was not eroded by penal taxation for the benefit of slackers and constant strikers as today. In our society, for all its waste on social services, the Christian virtue of charity has all but died out” (p. 30). This reverse Whiggism is unacceptable in historical writing.

Three of Bess's four marriages brought fortune through dower portions. Bess managed her growing estate well and is properly depicted as a “builder and dynast.” Rowse does not neglect the matrimonial and financial quarrels that arose between Shrewsbury and Bess. So enamored is Rowse of the countess that, although she and Shrewsbury were “about the same age” (Bess was actually a few years older than her fourth husband), Rowse refers to the earl as “old fool,” “old Earl,” and “old Shrewsbury” (p. 18), while Bess, who outlived Shrewsbury by about two decades, is never described as “old.” Rowse ends the brief biographical study of Bess by remembering a visit to her burial place. “There, in marble, she lies alone (where alone myself in the silence I have before now imprinted a chaste kiss on the cold forehead)” (p. 39).

The remaining four portraits are drawn more impartially. The most successful biographical study is that of Elizabeth's godson, Sir John Harington, epigrammatic writer and water closet advocate. Regarded by Elizabeth as a “lightweight,” Harington was never given a responsible position. Thus he was afforded the time to observe and write. Rowse presents a brief evaluation of Harington's works. In another sketch, Edward de Vere, seventeenth earl of Oxford, is portrayed as “gifted, neurotic, and unstable” (p. 76), and “really fit only for ceremonial occasions” (p. 103). Henry Cary, Lord Hunsdon and first cousin to Elizabeth, emerges as a faithful soldier, able administrator, and patron of the theater. Father Parsons (or Persons) is depicted as a brilliant but misguided Jesuit firebrand and sometime traitor. His attachment to Philip II and support for the Infanta Isabella as Elizabeth's successor are properly examined.

The author of *Bloody Mary* and *Great Harry*, a popular life of Henry VIII, has written a disappointing biography of Elizabeth I. Carolly Erickson is at her best dealing with the life of Elizabeth before 1558; fortunately, two-fifths of the book treat the years prior to accession. Elizabeth's training and education are extensively covered, as are the years at Hatfield, the Seymour episode, and her few months in the Tower during the Wyatt investigation. Erickson is particularly effective in describing the uneasy relationship that existed between Mary Tudor and Elizabeth.

For Elizabeth's reign, Erickson relies heavily on the *Calendar of Spanish Papers*, uncritically accepting the judgments of Feria and De Quadra. Gossip recorded in ambassadors' dispatches is accepted as evidence and, frequently, conclusion is based on conjecture. Not surprisingly, a most unfavorable characterization of Elizabeth virtually leaps from the pages of Erickson's book. A very substantial portion of the book's middle section is devoted to the relationship between Dudley and Elizabeth in the

early 1560s. We are informed that "no one could have predicted at the outset of the reign that Elizabeth would take Lord Robert for her lover" (p. 180) though "on the vital central issue whether or not the virgin queen lost her virginity in 1559 (if indeed she had not lost it to Thomas Seymour years earlier)—the records are silent" (p. 181). Erickson describes the relationship like soap-opera scenario. "She [Elizabeth] fondled Dudley like a lover in public" (p. 181). "Her feeling for him clearly went far beyond flirtation, far beyond passion (though only the most gullible can have accepted her urgent denial of their intimacy)" (p. 210).

The Elizabethan settlement of 1559 and other important matters of the early years of the reign are either inadequately treated or neglected. On page 184 we are told that "Protestants were a tiny minority in England in 1559," but two pages later Erickson states that "Catholic England was officially dead." The differences that developed between the queen and her Parliament on the particulars of the Elizabethan settlement are not treated. Scant attention is accorded the succession question of the 1560s. Erickson mistakenly refers to the 1566 Parliament as the third of the reign (it was the second session of Elizabeth's second Parliament). With respect to the Northern Rebellion the author states that "hundreds of thousands, conceivably even a majority of the English, were still Catholics" (p. 238). This high estimate of Catholic influence is repeated by Erickson in her review of the Jesuit mission. "The legal profession, the peerage, even to an extent the royal court were all strongholds of the ancient faith" (p. 333). Although Elizabethans perceived Roman Catholicism as a real threat, Wallace T. MacCaffrey notes that "the Catholics had no supporters in high places who actively sympathized with their cause. The Queen herself was perhaps their best friend" (*Queen Elizabeth and the Making of Policy, 1572–1588* [1981], p. 152).

Erickson's most prominent recurring theme is Elizabeth's sexual appetite. "She seduced men away from their wives. . . . She turned the royal court into a perfumed harem, sending away the dignified nobles of the old school and replacing them with dancing fops and lechers. . . . She was a disquieting anomaly at best; at worst she was a whore" (p. 268). Yet Walsingham, Knollys, and Burghley—neither fops nor lechers—were retained. Sir Christopher Hatton, characterized as Elizabeth's "moonstruck suitor" (p. 327), is regarded by MacCaffrey as an important and forceful policy maker.

Erickson's use of the episodic approach omits entire periods of Elizabeth's life and reign. Superficial treatment is accorded the Alençon match, but the serious foreign policy decisions of the 1580s are neglected. This impressionistic biography concludes with chapters on the Armada and Essex. One finds

little on Elizabeth for the years 1562–69, 1570–79, 1581–86, and 1590–97.

Erickson includes a quite full bibliography, but Collinson's *Elizabethan Puritan Movement* (1967) and MacCaffrey's volume, cited above, are notable omissions. Although Neale's biography remains the standard, the need persists for a serious biography of Elizabeth I (of the quality of J. J. Scarisbrick's *Henry VIII*) that incorporates the scholarship of the last three decades. A good biography certainly should illuminate character, but it must rest on historicism, not romanticism.

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WARREN L. CHERNAIK. *The Poet's Time: Politics and Religion in the Work of Andrew Marvell*. New York: Cambridge University Press. 1983. Pp. x, 249. \$37.50.

The appearance of the present book, some fifteen years after the publication of John M. Wallace's important work on Andrew Marvell, indicates that the controversy surrounding seventeenth-century English political history has now spread into the field of literary criticism. Wallace's highly regarded *Destiny His Choice: The Loyalty of Andrew Marvell* delineated a widespread loyalist mentality in revolutionary England that combined moderate principles of constitutional government with a willingness to switch sides during the changes of regimes between 1649 and 1688. Having established a loyalist tradition, Wallace then set out to place Marvell smack in the middle of it.

Warren L. Chernaik takes strong exception to Wallace's thesis, maintaining that the loyalist schema for Andrew Marvell makes him "simply unrecognizable" (p. 7). Indeed, *The Poet's Time: Politics and Religion in the Work of Andrew Marvell* has to be seen, at least in part, as a running commentary on the earlier Wallace book. For not only does Chernaik emphasize the irreconcilable disagreements polarizing political factions into pro- and antiabsolutist groupings, but also he devotes most of his attention to demonstrating how Marvell belongs to the latter.

Few would disagree with Chernaik's concluding remarks that sum up the poet's career as an "impressive and moving example of an artist's realism and courage in facing up to the problem of how to live in a fallen world" (p. 205). But the bulk of the book, in which he develops his own interpretation of Marvell's religion and politics based on a careful analysis of both poems and prose, is highly controversial. For Chernaik, the wellspring of Marvell's politics is religion, defined here as a Puritanism of the libertarian left. Stressing his religious commitment, the author then argues that Marvell, as a man

of firm principles, could never be an opportunist or a turncoat. Commands of conscience always took precedence over the commands of state. Moreover, his resistance to all forms of absolutism, stemming from an adherence to the liberal contract theory, remained a consistent theme throughout his life. Placing Marvell well within the framework of libertarian Puritanism and classical liberalism, the book then traces the development of the poet's political activities and writings, from his early ambivalence toward Cromwell as expressed in "An Horatio Ode" and "Upon Appleton House," to his open opposition to the Stuarts during the Restoration period. From this perspective, Andrew Marvell becomes an essential cog in the English liberal tradition from John Milton through to John Locke.

To this reader, the most persuasive aspect of the book's thesis pertains to Marvell's role as a leading member of the parliamentary opposition to Charles II. Less convincing is the explanation for the poet's shifts during the decade of the 1650s. Indeed, Marvell decided to link his fortunes with Cromwell at about the time that the Lord Protector was assuming a more authoritarian stance. The author also fails to explain fully why a supposed libertarian Puritan sharply criticized those sectarians and Levelers who one might expect to have been his natural allies. There are other questions that arise in this thoughtful and provocative book. Because it is well written and well researched, *The Poet's Time* forces us to reexamine accepted notions and, therefore, makes a valuable contribution to seventeenth-century studies.

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CHRISTOPHER HILL *et al.* *The World of the Muggletonians*. London: Temple Smith. 1983. Pp. 195. £12.50.

Like its companion volume, *The World of the Ranters* by A. L. Morton, this triple-authored book provides a thoughtful description and analysis of a fairly obscure mid-seventeenth-century Puritan sect called the Muggletonians. This sect was founded in 1652 by John Reeve, a London tailor, and his journeyman tailor cousin, Lodowick Muggleton, who had read the mystical writings of Jacob Boehme. The Muggletonians believed that their leaders were the Two Last Witnesses mentioned in the Book of Revelations. In an extreme manifestation of Calvinism, the leaders assumed the power to damn men to hell or to bless them to salvation. Although they shared many of the religious ideas that Christopher Hill detected in the "upside down" world including anti-trinitarianism, the mortality of the soul, and the

doctrine of the two seeds, the Muggletonians expelled from their midst three of the leading Ranter spokesmen, Lawrence Clarkson, John Robins, and Thomas Tany and quarreled sharply with the Quaker leaders, William Penn and George Fox.

The occasion for renewed interest in the Muggletonians is the recent discovery of the sect's papers, which the alleged last Muggletonian, Philip Noakes, salvaged during the London bombings of World War II. In chapter 5 of the book under review, and in a recent article in *Past and Present* (number 99 [1983])—which might well have been substituted for the volume's extraneous chapter on Clarkson—William Lamont has used the Noakes papers to answer, among others, the question, why the Muggletonians, like the Quakers, were the only seventeenth-century sectarians to survive into the twentieth century? After all, William Blake's mother may have been a Muggletonian; Walter Scott was damned to hell by a Muggletonian; Herman Melville knew of some in upper New York state; and Thomas Carlyle's sitting room was graced by a screen on which his wife pasted, among other pictures, that of Muggleton.

Lamont asserts that, after the death of Reeve in 1658, Muggleton substantially altered the sect's ideology in one important regard. According to Lamont, by arguing that God does not take "immediate notice" of the suffering of the saints nor of their prayers, the Muggletonians "gave up believing in an imminent transformation of society" (p. 126), a distinction that, Lamont believes, was the major difference between early and late Muggletonianism. The unsuccessful attempts of Thomas Robinson and others in the nineteenth century to reassert the Reevonian position on this point without rejecting other Muggletonian positions confirms the sect's willingness to adjust to the facts of the Restoration. It may well be that dropping the Reevonian idea permitted the Muggletonians, like the Quakers, to survive into the twentieth century, but surely one of the significant reasons why the Muggletonians, unlike the Quakers, remained such a tiny sect during the modern period has been the persistence of many of the extremist ideas that the sect seems to have held during the 1650s.

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JAMES R. JACOB. *Henry Stubbe, Radical Protestantism, and the Early Enlightenment*. New York: Cambridge University Press. 1983. Pp. viii, 222. \$37.50.

Henry Stubbe was a marvelous crank. Possessed of a sharp wit and a sharper pen, he enlisted in many of the pamphlet battles of the Interregnum and Resto-

ration. While at Oxford in the 1650s, he championed republicanism, religious independence, and the philosophy of Thomas Hobbes. After the Restoration he repudiated his republicanism and became first a spokesman for the court and then a mouthpiece of the country opposition. He also wrote a discourse extolling the qualities of chocolate as an aphrodisiac, a tract rationalizing the healing miracles of the Irish thaumaturgist Valentine Greatrakes, a manuscript praising the virtues of Moham-medanism over the corruptions of modern Christianity, and, famously, an assault on the Royal Society. Most historians who have examined Stubbe's career have concluded that he was a mercenary, ready to sell his services to the highest bidder. There is merit in this view. Stubbe's ostensible allegiances were spectacularly inconsistent, and he had the survivor's knack for retreating from exposed positions. Henry Stubbe, it would seem, was a man neither of philosophical constancy nor of personal integrity.

James R. Jacob will have none of this. His Henry Stubbe was a consistent advocate of radical religious and political views. According to Jacob, most of Stubbe's contemporaries and all previous historians have misunderstood his works. Beginning as a "Hobbesian Independent" in the Interregnum, Stubbe's opinions changed without moderating. Even after the Restoration, when he had become a crown propagandist, he sought to convey his heterodox views covertly. His tracts supporting the monarchy's foreign and religious policies actually contained a Hobbesian and Harringtonian critique of crown and church that was disguised by stylistic sleight of hand. His religious ideas became more subversive in the repressive atmosphere of the Restoration and prefigured the deism of Charles Blount and John Toland. Even Stubbe's defense of Greatrakes and attacks on the Royal Society display his consistent, though covert, radicalism. A "radical Galenist" and an Aristotelian of sorts, Stubbe was anxious in these works to advance his Arian Christianity and demolish the alliance that had been forged between natural philosophy and latitudinarian orthodoxy. In Jacob's view, therefore, Stubbe was a key figure, at once linking the radical religious and political thinkers of the English Revolution with their Enlightenment counterparts and also confirming the existence of the postrevolutionary partnership between science and establishment religion that Jacob and his collaborator Margaret Jacob have identified.

Jacob demonstrates persuasively that Stubbe's *volte face* after the Restoration was less complete than has been hitherto supposed. His interpretation of Stubbe's career also clarifies the thought of the major thinkers who were his heroes and adversaries. But Jacob's larger claims are doubtful. Stubbe's

views were too eccentric and contradictory to be representative of an underground movement that opposed the religious and scientific establishment. They were characteristic neither of radical Protestantism nor of the early Enlightenment. Besides, few of his contemporaries could have successfully decoded his radical messages: the key text remained in manuscript until 1911.

MICHAEL MACDONALD
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Madison

GEOFFREY BEARD. *Craftsmen and Interior Decoration in England, 1660–1820*. New York: Holmes and Meier. 1981. Pp. xxiv, 311. \$85.00.

The craftsmen who gave splendor to the great houses of England are a relatively neglected race: we tend to associate buildings with architects and overlook the men who created the distinctive beauty of the interiors, unless their name happens to be Robert Adam. The careers of craftsmen before the Restoration were poorly documented, with anonymity claiming too many victims. In any case, the range of skills at that time was not particularly well developed: important houses like Wilton and Bolsover had limited schemes of interior decoration because the applied arts and furniture design had not kept pace with architectural advances. The great generations of craftsmen began after the Restoration, from which time sufficient building records survive to permit one to follow the movements of these men from project to project and to form an idea of their cumulative achievement. Geoffrey Beard has gone through the records relating to most of the significant private construction schemes down to 1820 in order to track the activities of the master craftsmen across the country. He has compiled an extremely informative survey of their work. Royal and ecclesiastical commissions are largely excluded, since they have been well treated elsewhere, so the focus here is primarily on the decoration of the houses of the landed aristocracy.

Paradoxically, it was the Great Fire of London that most effectively advanced the crafts in England. When Christopher Wren was entrusted with the design for most of the churches and civic buildings, he was determined to assemble teams of men who could create modern interiors compatible with his architecture and, thus, dispel the notion that England always lagged a generation or two behind France and Italy in matters of design. Such was the amount of work to be undertaken that an act of 1666 lifted the restriction that required work in London be done only by members of the city guilds, and construction in the capital city was opened to provincial as well as foreign artists on a competitive

basis. St. Paul's provided the focus for this activity, but all parish churches benefited from the increasing skills of men working in stimulating cooperation on well-funded projects of national importance.

Beard demonstrates that it was not until the late 1680s that Wren's team had developed a comprehensive mastery of interior decoration. By then the art of woodcarving had reached incomparable heights: Grinling Gibbons, who seems to have received his training in Holland, had a sensitive virtuosity that has never since been matched, although Jonathan Maine and Edward Pierce, whose training had been in England, could almost rival his imaginative handling of wood. Jean Tijou from France executed marvels in wrought iron and effectively introduced into England a new craft, which soon became indispensable in country houses. John Grove, Henry Doogood, and later Edward Goudge, all masters of plasterwork, produced those richly encrusted ceilings so characteristic of Restoration interiors. The decorative painters Verrio and Laquerre moved to England and worked mainly for the nobility, whose walls and ceilings they covered with heroic fictions. Italian *stuccatori* introduced their art in the early eighteenth century, and workers in scagliola followed. The presence of these foreign artists meant that the novel skills and artistic graces admired by patrons on their travels abroad could be communicated to English craftsmen who had little opportunity to observe what was in vogue overseas. As the architects associated with Wren moved on to large new projects in the country, they took with them their top London craftsmen, who intermingled with the provincial masters. Thus, by the time we reach the Palladian revival of Burlington, Campbell, Flitcroft, and Kent, there existed a network of accomplished craftsmen able to furnish the houses of the nobility and gentry with decoration of outstanding quality. With the emergence of Robert Adam around 1760, English interior design could claim to be the finest in Europe.

The craftsmen were only part of a whole economy of construction. Beard expertly describes the materials required in the various crafts, the sources of supply, the division of labor and phases of work in eighteenth-century building schemes, and the interaction of craftsmen, architects, and patrons. It is essentially a factual account, with little attempt to explain the shifts in taste or to suggest the character of life within the various houses. Beard is drawing on his years of research into English craftsmanship that has already given us books on Georgian craftsmen, Robert Adam, and decorative plasterwork. The present work provides the most comprehensive survey of the subject available. It is consolidated by a select dictionary of craftsmen, which details their activities and their mention in records. The text is complemented by superb photographs in black and

white and in color by A. F. Kersting that demonstrate irresistibly why the interiors of this period will always command admiration and convey delight.

GRAHAM PARRY
University of York

RICHARD J. HARGROVE, JR. *General John Burgoyne*. Newark: University of Delaware Press. 1983. Pp. 294. \$28.50.

Richard S. Hargrove, Jr., has written the most recent of several studies of General John Burgoyne. A biography of Burgoyne by Gerald Howson appeared only four years ago. Hargrove does not provide the reader with any startling new information—there is already so much in print about Burgoyne—nor does his judgment of the general differ radically from that of other scholars, particularly George Billias in his perceptive article "John Burgoyne: Ambitious General," in *George Washington's Opponents*, edited by George Billias ([1969], pp. 142–92).

Hargrove's work does, however, have certain advantages over previous appraisals. He has used well the numerous secondary works and has viewed all important primary sources now available to scholars. His biography is objective and, in that respect, has an advantage over Howson's special pleading for "Gentleman Johnny." Hargrove contends that Burgoyne was most instrumental in drawing up the plans for the 1777 campaign, that he started it well, even brilliantly, with his pouncing on Ticonderoga, but then faltered. He had failed to provide adequate land transportation for provisions and forage, the shortage of which affected him desperately when the loyalists joined him later. He took the wrong route south from Skenesborough (Whitehall) to Fort Edward and wasted time and precious supplies to build a road through the wilderness. He foolishly cut off his own line of retreat and had no one to blame but himself (although Burgoyne never did blame himself, at least not publicly) for his surrender. These arguments are not new, but Hargrove backs them with a wealth of sources. His writing is clear and succinct, and his biography is, perhaps, the most readable of those that have appeared.

Although Hargrove's focus is Burgoyne (and covers admirably his career before and after the period spent in America) and not the whole New York campaign of 1777, he omits some considerations that might have added more fully to our understanding of Burgoyne's thinking between the battles of Ticonderoga and Saratoga. Hargrove, for instance, does not integrate the St. Leger campaign into his narrative, but mentions it only peripherally. Perhaps it did not figure importantly in Burgoyne's

thought, but the reader would appreciate an explanatory footnote or possibly an appendix on St. Leger, as well as on Stanwix and Oriskany. Also, Hargrove does not mention some important tactical events. The reader is told only about Colonel Friedrich Baum's defeat at Bennington, not about Colonel Heinrich von Breyman's. Burgoyne sent Breyman into battle. Breyman's troops marched too slowly and, as a result, suffered casualties; but the reader does not learn the circumstances of the action. Hargrove narrates the fight at Freeman's Farm on September 19, but does not describe at all the battle of October 7 in which Brigadier General Simon Fraser was killed. Indeed, the author provides far more detail on the circumstances of Fraser's burial than on those of his death. Since in other places Hargrove amply demonstrates Burgoyne's courage and tactical ability, his failure to appraise the general's leadership in the most important battle of his career makes his judgments on Burgoyne as tactician seem curiously incomplete.

These reservations aside, Hargrove's is a sound, scholarly, readable biography.

FRANKLIN B. WICKWIRE
University of Massachusetts

JOHN BOHSTEDT. *Riots and Community Politics in England and Wales, 1790–1810*. Cambridge: Harvard University Press. 1983. Pp. viii, 310. \$37.50.

John Bohstedt's book will engage both those for whom history is still a narrative and those who wish to have hypotheses explicitly tested. In chapter 1 "riots" and "communities" are carefully defined. The period covered is admitted to be mainly arbitrary, although one in which rioting was frequent, ending just before the much-studied Luddite riots. The study is based on a national sample of 617 riots reported in Home Office files, two major newspapers, and the *Annual Register's* chronologies of events. Thirty nine percent of the riots revolved around the issue of food, 22 percent were militia riots, 10 percent politically or ideologically motivated riots, and 7 percent labor riots, leaving 22 percent in miscellaneous categories.

The three major case studies are concerned respectively with Devon food riots; with Manchester, where politics rides high; and with a broad sweep of rural disturbances, among which the militia riots, especially those in Lincolnshire, are prominent. Not only are there contrasts in the issues but also in the way the rioters behaved and in the ways the riots related to community politics. All, however, show that the hardship model of protest is hardly ever a sufficient explanation of rioting.

The Devon study demonstrates how a riot could be used as a carefully controlled weapon by people

who were not of the lowest classes yet felt the pinch of sharply rising food prices. The most successful riots tended to occur in large industrial villages and small market towns, often self-governing parliamentary boroughs. Here there was a continuing tradition of face-to-face marketing that enabled rioters to control food supplies and to lower prices. Moreover, the riot leaders knew most of their men personally, and this enabled discipline to be maintained.

Manchester was quite different. Being a large industrial town, its growth was such that the local authorities hardly knew what was going on, while mobs rapidly got out of control for similar reasons. Food riots were unsuccessful because supplies came from many points, mostly distant, and through middlemen. Here political and ideological riots developed as a reaction to the hopelessness of other forms of protest, thus forming a basis for class politics.

By comparison, most parts of rural England were quiet, because horizontal networks were strong, as in small-town Devon, but so were the vertical networks, which acted as a restraint on the development of a tradition of ordered rioting. Therefore, the militia riots, in which young men protested against conscription into a part-time reserve army, were a correspondingly abrupt and fierce reaction against the government and the county lieutenancy, distant figures compared with local farmers and squires.

The relative quietness of rural England may, as Bohstedt admits, be partly an artifact of his definition of a riot as a disturbance involving a minimum of fifty persons. This is, perhaps, the one significant weakness in his analysis, but Bohstedt has provided a clear framework within which more detailed studies can be undertaken to modify or confirm his interesting conclusions.

DENNIS MILLS
Open University

MARY PRIOR. *Fisher Row: Fishermen, Bargemen, and Canal Boatmen in Oxford, 1500–1900*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xxiv, 406. \$54.00.

This is a richly detailed study of a tiny riverside community, one which was dependent on a group of interlinked occupations. In many English local histories, the place is the first consideration and the historian stays within certain boundaries. In this book by Mary Prior, the people and their families come first. It is, in many respects, a pioneering tour de force that answers important questions about transport, family relationships and status, and the crafts from which the community drew sustenance. It succeeds, in the course of an unfolding narrative

covering several centuries, in shedding light on the transport and economic history of the Upper Thames Valley and on the town of Oxford itself.

The Fisher Row locality, developing into several rows of houses near what is now Hythe Bridge, Oxford, was the home of Thames fishermen during the Tudor period and possibly during the late Middle Ages as well. These men were ultimately, as Prior shows, at the mercy of technology and general economic conditions: medieval corn-mill building brought "the politics of water-power" and associated economic and social conflicts, and the opening of the Oxford Canal (1790) interrupted the settled ways of life of the established local bargemen and boatmen. Later, the expansion of the trade in sea fish, stimulated by the rise of the railway system, destroyed the livelihoods of river fishermen. The author discusses how the latter pursued their craft, which was originally dependent on religious fast days, and illuminates the work of the bargemen (river boatmen) and the problems they encountered when navigating between Oxford and London before the opening of the canal. The untold thousands of us who have been nourished—or driven into malnutrition—by the bare generalizations of transport history will find this book nearly compulsive reading. It should certainly belong to the enforced variety.

Much of the information in the book has been wrung from intractable sources, and long sections of the narrative contain implicit and explicit discussions of methodology. These passages are often fascinating. Unfortunately, the pursuit and reconstitution of family history has entailed almost equally long and "dense thicket of genealogy," as Prior aptly calls them (p. 170); but, of course, one has to demonstrate the continued existence of families in order to discuss them at all. The book, in consequence, contains inevitable longueurs. Yet, the immense and highly original work of reconstitution compels respect, the more so because the author has presented the families not as a random mass of persons in a parish register but in relation to the property, commitments, and organization of distinct occupational groups. Prior is frank about the dangers she has encountered. She is sometimes less than convincing about the precise status and wealth of her leading families, partly because the economic structure of artisan or trading families in her region has been inadequately explored, and partly because probate inventories are not very numerous and disappear by the 1730s. But her excursion into the nineteenth century brings some striking sociodemographic discoveries: by 1851, for example, boatmen's families were matrilocal and extended, and the men were hardly ever at home; whereas fishermen's families were patriarchal and nuclear.

There are some illuminating asides on the role of

public houses in the lives of the boatmen, and there is a moving discussion of the decline of the Fisher Row community in the nineteenth and twentieth centuries. Little of it now remains. The author has demonstrated, triumphantly, the virtues and effectiveness of microhistory, which, however, must be pursued thoroughly and imaginatively if it is to be done at all. There are some useful diagrams, and the book is presented and written in a pleasant, unforced style.

J. D. MARSHALL

University of Lancaster

HENRY WEISSER. *April 10: Challenge and Response in England in 1848*. New York: University Press of America. 1983. Pp. xiii, 329. Cloth \$23.50, paper \$13.00.

Interest in Chartism continues unabated and the focus is narrowing. The most recent study of the topic, *London Chartist 1838–1848* (1982) by David Goodway, is now followed by Henry Weisser's book dealing with the final year of the Chartist movement. More particularly, Weisser focuses on April 10, 1848, the day of the huge Kennington Common meeting planned as the staging area for the procession to Parliament to present the third and last Chartist petition.

Weisser's basic sources are Home Office papers and newspapers. There are ample quotes from several of the Chartist papers and from the *Times* and *Weekly Dispatch*. They give the reader a feeling of the immediacy of events before, during, and after April 10. And there is empathy here, too; a kind of lamentation for a lost cause. The themes of the book are challenge and response, and the discussion centers on the interaction of challenges and responses put forth by both the Chartists and the government. Rumor and myth are an integral part of the crisis, too. Both sides often acted out of fear: fear on the part of the upper classes that the mob would destroy property, and fear on the part of the Chartists that the government intended to massacre them.

There is little here that is new in the way of facts or interpretation. Weisser says at the beginning of the book that he is not making "a demonstration on behalf of one or another ideology" (p. xi) and in closing states that despite "the newer research on Chartism . . . the standard interpretation [that] . . . middle class support for the regime was too powerful and Chartist support for an insurrection was too weak" (p. 300) remains in place.

The book has value, nevertheless. It is the most detailed account of April 10 in one volume yet written, and the author is fully abreast of the most recent research. John Saville is singled out as begin-

ning the revisionist interpretation of Chartism by moving away from the myth of the "April 10 fiasco" and also in calling for the rehabilitation of Feargus O'Connor, who deserves respect as a national leader rather than the ridicule he has received. Although defending O'Connor, Weisser does lapse into stereotype by saying that O'Connor found "relief in flying into melodrama, a style of escape often favored by the Irish" (p. 86).

The discussion of developments after April 10 is fresher and more provocative than the discussion of April 10 itself. Here, only Goodway has anticipated Weisser in detail and zest. Weisser's familiarity with local Chartist sources pays off with a vivid portrait of Bradford Chartist meetings in June. Regarding the insurrectionaries of August, Weisser defends Thomas Frost as a source, whereas others have found Frost unreliable, but he accepts the idea that Cuffay's role has been exaggerated. There is a good discussion here of the rhetoric of violence versus the reality of revolution. The futility of insurrectionary plans is a consistent theme of the book. In this and in putting forward the myth of Ernest Jones, the author is openly at odds with the interpretation of the left.

NORBERT J. GOSSMAN
University of Detroit

STEVEN MINTZ. *A Prison of Expectations: The Family in Victorian Culture*. New York: New York University Press. 1983. Pp. xiii, 234. \$25.00.

Although a great deal has already been written about the history of Western families, much still needs exploration. Apart from sorting out the problem of the reliability of evidence and differentiating family behavior by ranks or classes, other pivotal questions remain. Paramount among these is the problem of how the family mediates between cultural forces and the characteristics of individuals. It is to this difficult issue that Steven Mintz addresses his book. It stands out as one of the few successful efforts to investigate in some detail this family nexus between culture and individual lives.

Mintz centers his study on five prominent Victorian writers, Robert Louis Stevenson, George Eliot, Harriet Beecher Stowe, Catherine Sedgwick, and Samuel Butler, all born between 1789 and 1850. His thesis is that far from being raised in a home environment that was "a walled garden"—a metaphor frequently used to describe the Victorian family's isolation from the outside world—the emotional dynamics, religious ideas, and value orientations of these five Victorian families show that they interacted with society outside of the family and were like sponges that absorbed deeply the climate of their times. The home life of these literary figures

and, consequently, the results of their socialization in this milieu reflected the drama of ideas then current. Victorianism permeated the tone of family discussions, affected its semantic expression of sentiment, and provided structure and content to the intergenerational conflicts within each of the homes. Mintz illustrates that the domestic conflicts largely entailed a clash between an older world system, in which values of authority, tradition, and obedience were championed by the father, and a newer world system advocated by the children, which stressed individuality, evangelical religion, and material success. All of these writers believed that they must determine their own destinies and rejected much of the advice and authority of their fathers. Yet, none of this went so far as to jeopardize the essential emotional bonds or essential hierarchical structure of their families. Victorians relied on the stability of the family to prevent the newer values from destroying the older ones. The reliance was well placed, for these families maintained stability through a process of intergenerational accommodation and compromise.

The chapters delving inside the family are particularly outstanding in their descriptions and analyses of the manner in which familial and individual emotional conflict reflected aspects of Victorianism. In the method employed to isolate Victorian characteristics, however, Mintz relies on the term "middle class" to distinguish his group of Victorians from others. But in what way were they middle class? It is not that he is unaware of the scholarly wrangles over the term, which he does discuss in an appendix. But it would have been better to explain and integrate the argument within the text so that the reader could be aware of the way Mintz intended the term to be understood.

Finally, a few quibbles about facts. It is still an unsettled question that "by the second quarter of the nineteenth century in America, and by the 1850's in England, growing numbers of married women began *consciously* [my emphasis] to have fewer children," as Mintz maintains (p. 16). A relatively new study reflects the controversy (M. Davies, "Corsets and Conceptions: Fashions and Demographic Trends in the Nineteenth Century," *Comparative Study of History and Society* [1982]: especially pp. 611–16). In addition, Mintz's statement that "by the early nineteenth century a marked decline had occurred in infant and child mortality" (p. 19) cannot be easily verified since it was only after 1840 that firm evidence about infant mortality existed for England. After 1840 such a "marked decline" can only be firmly asserted for the years after 1906–10 (see F. B. Smith, *The People's Health, 1830–1910* [1979], p. 65).

But these are only quibbles; they do not diminish the importance or originality of the book. In conception, methodology, and substance, Mintz has

made an important contribution in revealing the family as the bridge between culture and the individual.

VIVIAN C. FOX
Bunker Hill Community College

JOEL H. WIENER. *Radicalism and Freethought in Nineteenth-Century Britain: The Life of Richard Carlile*. (Contributions in Labor History, number 13.) Westport, Conn.: Greenwood. 1983. Pp. x, 285. \$29.95.

Joel H. Wiener, author of a work on the movement in the 1830s to repeal the British newspaper tax, has followed that study up with a biography of Richard Carlile, the principal figure in the agitation for a free press. The result is a sound and perceptive book that supersedes the rather thin studies of Carlile previously written. The lack of an elegant style and of the use of intimate sources, however, prevent its being the pioneering political biography that the author says he hoped to produce.

Carlile's two consuming interests in life were freethought and a free press. Both causes were served by the fact that he was, as noted by Francis Place and other observers, "a single-minded honest-hearted fanatic." He sought out controversial causes that he could serve in his capacity as a printer and editor. He was prepared to test the government and the courts over the vague laws regulating blasphemy, sedition, and taste. Imprisoned several times, he lengthened his sentence on each occasion by refusing to pay the fines and give the sureties that were imposed along with the jail terms. But he also had limitations. He was poorly educated, as Wiener comments, "even by the least enlightened standards of the time." He was a conduit for ideas rather than an original thinker. He was a strong moralist, as were most radicals, but a publicly fallen one. He lived above his means whenever possible, and he discarded his wife and took a mistress in what he lamely termed a "moral marriage." In his politics he was an eighteenth-century crusader for individual rights rather than a nineteenth-century democratic reformer of society. His enemies were the old ones of kings, lords, and bishops, groups he termed, in a phrase that reveals both his vigor and lack of finesse with words, "a huge tapeworm ratifying itself throughout the stomach of the body politic."

Unfortunately for Carlile, his two great causes gradually merged and negated each other. Wiener correctly emphasizes this aspect of the radical's career. Carlile's freethought moved from deism to militant atheism, a phase in which he accomplished most for the cause of a free press, to a murky "allegorical" Christianity that only he understood. With evangelical zeal and personal courage he increasingly devoted his publishing and private ener-

gies to the furtherance of oddball religious views that ended his effectiveness as a political crusader.

Carlile's papers in the Huntington Library are inadequate to support a full biography, so Wiener has had to winnow most of the personal material on his subject from the agitator's own writings. He has done a thorough job, and not much else is likely to be said about Carlile's life and career. In this light, it should be noted that the book is professional also in the more limited sense of the word: it is written in standard academic prose and is of interest mainly to specialists in early nineteenth-century British history. Within those usual limits, it is a welcome addition to the historian's bookshelf.

CARLOS FLICK
Mercer University

DIANA DAVIDS OLIEN. *Morpeth: A Victorian Public Career*. Washington: University Press of America. 1983. Pp. xii, 526. Cloth \$29.75, paper \$18.75.

In an evaluation of his chief secretary for Ireland, whom he had recently elevated to the cabinet, Lord Melbourne observed that Viscount Morpeth's only fault was that he could "deny no one anything that is asked." This "softness of disposition" would be an "impediment" to a career in politics (p. 205).

George William Frederick Howard, Viscount Morpeth, and seventh Earl of Carlisle after 1848, was born in 1802 into one of England's great Whig families. His mother, the major influence in his life, taught Christian morality and responsibility to her children. As a result of these maternal lessons and of his formal education at Eton and Oxford, the young nobleman became convinced that his wealth and social position could only be justified through public service.

Utilizing the letters and diaries at Castle Howard, Diana Davids Olien has completed a useful study of this progressive Whig aristocrat who participated in the affairs of state from his first election to the House of Commons in 1826 until his death in 1864 just after resigning as lord lieutenant of Ireland. In view of his later involvement with the Irish, it was appropriate that his maiden speech should be a plea for Catholic Emancipation.

Morpeth's interest in the newly urbanized and industrialized England intensified when he left a safe family borough to represent the West Riding of Yorkshire. Vigorously supporting public-health measures and better housing for working people, he hoped to obliterate dirt and disease. Active in educational projects and the reform of juvenile criminals, Morpeth established a model reformatory on his own estate. A firm believer in freedom of worship, opinion, and commerce, Morpeth was confident that England could be a united community even

though vast inequities remained in the distribution of wealth. While many recall the generous policies of Irish Undersecretary Thomas Drummond, few are aware that Morpeth, chief secretary between 1835 and 1841, boldly advocated justice for Ireland's Catholic population.

Defeated in 1841, Morpeth made a fatal decision that removed him from the first rank of Whig politicians. Instead of promptly finding a new seat, he embarked on a one-year journey to the United States. He lacked the ambition, vanity, and toughness of a Lord John Russell or a Viscount Palmerston to make a successful political comeback. Olien includes fascinating material about Morpeth's American visit, an earlier trip to Russia for the coronation of Nicholas I, and his later year-long pilgrimage to the Ottoman empire.

Joining Russell's cabinet in 1846, Morpeth was disappointed to be put in charge of woods and forests. He was able, however, to collaborate with Edwin Chadwick in formulating the Public Health Act of 1848. Increasingly distressed by Russell's policies, Morpeth did not regret the government's defeat in 1852. But he was hurt when ignored by the Aberdeen coalition and by his apparent exclusion from Palmerston's administration in 1855. Then a series of unexpected resignations gave Morpeth the position he coveted, lord lieutenant of Ireland. He plunged joyfully into his largely ceremonial chores; for much of the next nine years Morpeth listened to deputations and visited schools, prisons, workhouses, and hospitals in order to demonstrate his commitment to betterment. Despite surviving the herculean banquet over which he presided to commemorate Shakespeare's tercentenary, Morpeth's health failed rapidly and he died shortly thereafter.

This fine biography portrays a well-intentioned, high-minded Victorian nobleman who truly believed that he was morally obligated to improve the world in which he lived but, as Francis Baring noted in that classic definition of Whiggery, "not to extremities."

THOMAS J. SPINNER, JR.
University of Vermont

PAUL T. PHILLIPS. *The Sectarian Spirit: Sectarianism, Society, and Politics in Victorian Cotton Towns*. Buffalo: University of Toronto Press. 1982. Pp. x. 210. \$30.00.

This study of the politics of religious sectarianism in four Lancashire cotton towns during the nineteenth century is an interesting survey that has aspirations to be much more. At its best, it is a valuable inquiry into the intermeshing of religious affiliation, sectarian concerns, and municipal and electoral politics in

Preston, Bolton, Stockport, and Blackburn. But the deployment of data—especially on church attendance and support—is somewhat wooden, and the book is not easily read because its detailed information is lightened only by occasional and very general formulations of the overall argument. Nevertheless, by providing that detail and by uncovering the interrelationship of political and religious conflict at the local level, Paul T. Phillips has written a useful book that tells us much, in particular, about bourgeois and petit-bourgeois religious politics.

In its greater ambitions the book is less successful. Phillips is seeking a precise (though, he argues firmly, nonquantitative) assessment of the influence of sectarianism on social and political relations in the four towns. He strives to determine the extent to which sectarianism cut across class lines (the touchstone seems to be whether both Anglicans and Dissenters were led by members of the wealthy elite) and, by implication, reduced the manifestation of class conflict. His conclusion is that sectarian conflict departed from class lines everywhere except in Preston, where the elite remained united, denominational conflict did not count, and sectarianism in the form of popular teetotalism actually served to reinforce rather than to undermine class divisions. The argument of the book, as opposed to the often interesting material that dominates its pages, is too simple. Above all, class and religious sectarianism are set against each other with a deceptive clarity that leaves the living reality of the interpenetration of religious and social forces in people's lives neither confronted nor explored. Evidence for class divisions and relations, for example, is sought only in politics. Phillips's social categories, to take another example, are broad (manufacturers, shopkeepers, and so on), and he deduces an absence of economic interest behind other allegiances if he can show that each of these very general groups is divided in politics and religion. A local study could surely be more imaginative in its questions and sources: Did Nonconformist mill owners or shopkeepers differ from Anglican ones? Butchers from grocers? Tory shopkeepers from liberal ones? The challenge of the book's hypothesis demanded a subtle uncovering of the textures of relationships in each town if it was to be met, but we are not given that. It is as a study of sectarianism, rather than as an attempt to measure the impact of sectarianism on class relations, that this book will prove useful.

GEOFFREY CROSSICK
University of Essex

MELANIE TEBBUTT. *Making Ends Meet: Pawnbroking and Working-Class Credit*. Leicester: Leicester University Press or St. Martin's Press, New York. 1983. Pp. 235. \$27.50.

For the working classes of Victorian and Edwardian industrial towns, ports, and big cities, the pawnbroker's shop played as regular a role in their weekly routine as that of church, chapel, club, or trade union. It was the nearest thing they had to a bank. They regularly deposited some of their most valued goods—best clothes would be pawned weekly, blankets or sports gear seasonally, and wedding certificates, false teeth, tools, or beds in a time of crisis—in return for cash advances. In continental Europe, church or state-backed *monts-de-piété* were created to provide credit for the urban poor. In Britain the same vital role was left to small-scale free enterprise in the form of the pawnbroker's shop. Yet, Melanie Tebbutt's meticulous study represents the first serious historical research on pawnbroking and the working class.

Drawing especially on trade journals, business records of pawnbroking firms in the Manchester area, and reminiscences, Tebbutt provides many fascinating details for the social historian. The chief strength of the book is not, however, what it has to say about "making ends meet" in the working class. It is, rather, a study of a trade. Pawnbrokers lived off working-class vulnerability. They made their highest rates of profit on their smallest loans. Some of their victims deviously fought back. Pawnbrokers were sometimes sued, and there was a substantial trade in cheating pawnbrokers by faking pledge cards. Pawnbrokers were themselves involved in a black market of counterfeit pledge tickets, despite the aspirations of their leaders to the respectability of suburban Nonconformity. In the end, pawnbrokers found themselves overtaken in the interwar years by new forms of credit-hire purchase and club trading, which had less tarnished images. Women were always the bulk of the pawnshop customers, for they managed working-class family budgets. When the younger generation of women voted against it with their feet, pawnbroking as a trade was doomed.

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MADELEINE GINSBURG. *Victorian Dress in Photographs*. New York: Holmes and Meier. 1982. Pp. 192. \$42.50.

COLIN FORD and BRIAN HARRISON. *A Hundred Years Ago: Britain in the 1880s in Words and Photographs*. Cambridge: Harvard University Press. 1983. Pp. 335. \$25.00.

The use of contemporary photographs as a source of historical evidence has an obvious appeal. The photograph can add a dimension that no other source can provide: it fleshes out fact and narrative

with people, places, and objects seen not with our retrospective vision, but as contemporaries saw them. In practice, however, this potential is often unfulfilled. All too frequently, published collections of early photographs are of limited value and as tedious, and often even less informative, as someone else's family album. These two books represent both ends of the spectrum.

Madeleine Ginsburg's book is limited in scope, setting out to provide precise details and precise dates of Victorian clothing. While this is doubtless of great value to historians of fashion or of the evolution of dress, it has limited usefulness for the historian whose interests are somewhat broader. Fashion details can provide a useful tool for dating photographs, but, as soon becomes clear, not all prosperous people of the nineteenth century were concerned about up-to-date clothing. As a result, while the general trends are obvious, the use of fashion details to determine precise dates is limited. It requires an eye acutely attuned to differences in the height of a top hat or the set of a sleeve and a degree of skill in distinguishing between one bustle or hairstyle and another that most historians would find neither the time nor the inclination to acquire.

The attention to fine details and the narrow focus on clothing, which will doubtless appeal to the specialist, make Ginsburg's book less than appealing to the historian. Most of the photographs it contains are carefully and formally posed portraits of individuals and groups. There is little concern for the broader social setting or for an interpretation of the period that would add to the book's value for the less specialized student of the nineteenth century. The author's single-minded devotion to clothing and exhaustively detailed commentary on line, cut, and material are occasionally exercised at the expense of objectivity and accuracy in other areas. For example, those wearing the clothes (particularly the "ladies") are often chided by the author for their lack of fashion sense or their stubbornness in clinging to out-of-date styles, as if they had willfully failed in their central duty of acting as clotheshorses for twentieth-century investigators; a five-year-old boy, obviously a member of the prosperous elite in his light tunic, frilled drawers, and fashionable shoes, is asserted to be wearing "normal dress for the average child" (p. 102). With less than one-half of the book devoted to children's, men's, occupational, and regional clothing, there is an inevitable bias toward the study of the clothing of prosperous females. Given the stated restriction of its interests and objectives, it is perhaps unfair to criticize this work for its limitations and for the additional problems that they generate. Nevertheless, it is disappointing and will do nothing to convince the skeptics that photographs can be a valuable source of historical evidence.

The surface appearance and title of the work by Colin Ford and Brian Harrison suggest that this might be a book for the coffee table, possibly frivolous and produced for its display value rather than as a serious piece of historical work. It is aesthetically appealing, well designed, and visually interesting, but it is also a serious history of Britain in the 1880s. The authors ranged widely in their search for photographs and make extensive use of a wide variety of both primary and secondary sources. The book is well footnoted, and a brief, usually good, bibliographical essay is provided for each chapter.

Despite the remaining technical limitations, the photographers of the 1880s managed to capture a surprisingly wide range of activities, places, and people. Although they were reluctant to claim the "artistic" merit generally accorded to painters, many photographers produced works that have considerable aesthetic as well as historical value. The basket maker (p. 123) and the girl fisher (p. 8) have a clarity, immediacy, and appeal that would stand on equal footing with any twentieth-century photograph, while the fire brigade of Girton College (p. 65) is posed with care and an eye for design that does credit to its creator, even if it did nothing to relieve the apparent boredom of the subjects.

An introductory chapter deals with the assets and limitations of photographs as sources and sets photography within the technical and social context of the 1880s. While the life cycle of the individual is a central theme (there are chapters on childhood, work, home and family, illness, and old age), the authors broaden their scope with chapters on such topics as regional loyalties, social tensions, and pomp and circumstances to provide a broad overview of the 1880s. Ireland, trade unions, triumphal arches, Bernardo's children, butchers' shops, politics and politicians, prisons and preachers, even the rare glimpse of a smiling Victoria find a place in this well-constructed pattern, which pays careful attention to the forces of both continuity and change. This is not a summary history of the 1880s with photographs added, but rather history written from a variety of sources that skillfully blends in the added dimension of photography.

At times, the organization appears erratic and the material oddly unbalanced. For example, the theme of the individual life cycle is well handled and well developed; but by breaking it up, presumably so that the book could open with childhood and end with death, the authors employ an artifice that detracts from its impact. It is good to see a chapter on home and family that deals with more than carefully posed patriarchs surrounded by dutiful families, but this piece goes a little too far, perhaps, in attempting to redress the balance by placing overwhelming emphasis on the homes of the poor

and the vagrant and on masturbation, homosexuality, and prostitution. Such criticisms do not detract from the overall value of the book, which can be used profitably and with pleasure by historian and layman alike.

In sum, these works show that photographs share assets and liabilities common to most forms of historical evidence. In and of themselves, combined with no other sources and subject to inadequate interpretation, photographs have little to offer beyond an antiquarian and somewhat trivial interest. Used in combination with other materials and subject to careful analytical treatment, they can be a useful tool in the historian's toolbox and add a valuable dimension to our understanding of the past.

JANET ROEBUCK
University of New Mexico

KATRINA HONEYMAN. *Origins of Enterprise: Business Leadership in the Industrial Revolution*. New York: St. Martin's Press. 1982. Pp. vii, 204. \$20.00.

Katrina Honeyman sets out to examine the economic and social origins of English business leaders during the Industrial Revolution. She claims that it is a commonly held hypothesis "that the positions of industrial power in the eighteenth and early nineteenth centuries were open to individuals from diverse social origins, and particularly that unprecedented opportunities existed then for small men to attain the role of the entrepreneur" (p. 10). She tests the hypothesis against evidence drawn from the lead-mining, cotton-spinning, and lace-making industries, and finds it wanting. The bulk of entrepreneurs were not small men but men of moderate wealth, usually with prior experience in their industry or a related one and with social backgrounds similar to earlier generations of business leaders. Honeyman concludes that "despite the transformations of the economy and society c1750–c1830, there appears to have been little real change in industrial leadership" (p. 166).

These conclusions lead Honeyman to consider both the origins of the hypothesis and the reasons why it is wrong. Samuel Smiles is credited not with creating the myth of the self-made man, but with popularizing it: "industrialization had produced so many unpleasant consequences for the working man that the notion of a beneficial concomitant was likely to be well received" (p. 160). This does not, of course, explain why, according to Honeyman, modern historians as distinguished as Peter Mathias, François Crouzet, and Harold J. Perkin continue to subscribe to it. The truth is that they do not; thus, when the author attributes Smilesian views to them, she is creating straw men to knock down. As she

remarks more than once, the continuity of business leadership during the Industrial Revolution is not surprising.

But why did the small man fail to prosper? Fixed capital requirements were modest; but the needs for variable capital were large and unpredictable, and newcomers with limited resources could rarely afford them. The best ways of raising money were to form partnerships with business associates and to borrow from rich relations. These strategies were closed to small men of humble origins who had to rely on uncertain financing made through the precarious banking system. Honeyman also suggests, but does not prove, that it was helpful for successful entrepreneurs to possess backgrounds similar to men already in the business. Once again, the poor newcomer found the going tough.

The most valuable parts of this book are the collective biographies of several hundred entrepreneurs in the three industries investigated and the points of contrast among the industries themselves. They complement one another well: lead mining was capital intensive, lace making was modest in its capital needs, and cotton spinning fell somewhere in between. The evidence is clearly set out and the points are well made. Lead, cotton, and lace, however, are not the whole story of the Industrial Revolution. Would other industries show a similar pattern? And Honeyman does reveal that a small number of humble men managed to succeed. Possibly the economy was more open after 1750 than earlier; the point deserves consideration. Still, it is good to have a book on the origins of industrial enterprise based on empirical research rather than resting on the currently fashionable assertions of proto-industrialization.

L. A. CLARKSON
Queen's University of Belfast

KENNETH D. WALD. *Crosses on the Ballot: Patterns of British Voter Alignment since 1885*. Princeton: Princeton University Press. 1983. Pp. xvi, 263. \$25.00.

The declared object of this book is to investigate the relationship between voting and certain social groups in Britain between 1885 and 1914. After a critical review of the inadequacies of earlier work in the field, Kenneth D. Wald presents statistics on the working class and religion and correlates them with Conservative and unionist votes in general elections. He tends toward the conclusion that religion was a more important predictor of political loyalties than class and largely refutes the idea of class-based politics in pre-1914 Britain.

In his discussion of class, Wald makes use of voting statistics from municipal elections in several towns for which he has a measure of the wealth of

each ward. This leads him to conclude that class was not strongly related to voting, for Conservative strength did not differ markedly between poor and wealthy wards. He is not troubled by the fact that in poor wards the residents were unlikely to be voters. Indeed, he says: "The local government electorate was larger than the parliamentary electorate and presumably more representative of mass opinion" (p. 48). This overlooks the fact that lodgers (about 13 or 14 percent of parliamentary voters) were excluded from the municipal register, while over one-half million women with a property qualification were municipal but not, of course, parliamentary electors. Not only were the two electorates different, but the local one was also considerably *less representative* of the working class. Such a basic error should not have survived a doctoral thesis and a book.

Confronted with the empirical findings of David Butler and Donald Stokes that pre-1918 voters still alive in the 1960s showed a class rather than a religious loyalty, Wald suggests that when these voters were young men and women their views were not yet firm. This misses the point. Voters who were seventy in 1910 grew up when religion was a lively political question, while the young Edwardians, still alive in 1960, experienced a very different political debate. No advocate of class voting suggests it was dominant in 1885, only that it tended to become so by 1914 rather than after. The author is too fond of setting up straw men and is insufficiently aware of chronology.

In his central section Wald claims the primacy of religion, but his only firm evidence is that Nonconformity was a strongly negative predictor of Conservatism—scarcely a surprising finding. He finds that Anglicanism was not a positive predictor of Conservative voting, whereas Catholicism was. He rightly explains the latter by pointing out that, while many Irish Catholics were nonvoters, they provoked Protestants in their localities to vote Conservative. This is a valuable warning about mere statistical relationships. He divides the working class into six groups, each correlated with Conservative voting. But in order to test the strength of class voting, it would have been preferable to use Liberal-Labour voting because working-class Conservatism reflected a variety of circumstances that overrode class. Since only three of the six groups are consistent with class voting, the author rejects this interpretation. It is scarcely surprising, however, that agricultural workers were less class oriented than industrial workers. Faced with another obvious finding, that organized labor correlated negatively with Conservatism, he says that organized labor was only a small part of the population. That depends on when one looks. It is true enough for the 1880s, but by 1914 there were well over four million trade unionists.

At best the considerable work that has gone into this book confirms several already well-known relationships, but it gets us no further forward in the interpretation of British electoral patterns.

M. D. PUGH

University of Newcastle-upon-Tyne

P. A. HOWELL. *The Judicial Committee of the Privy Council, 1833–1876: Its Origins, Structure, and Development*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1979. Pp. xiv, 262. \$37.50.

The Judicial Committee of the Privy Council possessed final appellate jurisdiction for civil disputes that arose within the British empire. Founded in 1833 to replace the obsolete Appeals Committee, the Judicial Committee brought a more professional spirit to its business, primarily through recruitment of qualified judges. The committee soon acquired a reputation for probity in Great Britain, perhaps because its decisions did not have great domestic impact and, save some religious issues, did not arouse notoriety among the general public.

P. A. Howell has written a straightforward account of the origins, structure, and operations of the Judicial Committee. The author discusses in detail the genesis of appeals, the rules governing their validity, and the attempts to make the appeal process serve the interests of justice. He devotes considerable attention to the membership of the committee, particularly those individuals who contributed a significant proportion of their time to hearing appeals. Participation imposed heavy burdens, for the diversity of the empire meant a wide spectrum of legal systems from which appeals might originate. Familiarity with foreign systems of law was not a strong point of the judiciary, so service became that much more onerous. Despite the inherent problems, Howell concludes that, on balance, the committee succeeded in the attempt to resolve issues on appeal fairly and quickly.

This book is primarily an administrative history of the Judicial Committee; it does not deal with the cases on appeal, the legal issues raised, the appellate decisions, or the historical ramifications of the final outcomes. The committee attempted to provide natural justice within the common-law framework, a Blackstonian legacy that survived into the era of analytic jurisprudence. The varieties of juristic response produced by the diversity of legal problems the committee confronted made a valuable contribution to the legal development that English jurists thought particularly their own.

The purpose of this book is to present a portrait of the Judicial Committee at work in service to a global empire. In this task the author has succeeded.

Nevertheless, it should be clear that the monograph will appeal to specialists; it is unlikely that it would have relevance for those not interested in English history. Its narrow focus notwithstanding, this volume does provide an excellent account of an institution whose work had previously been shrouded in mystery.

RICHARD A. COSGROVE

University of Arizona

SYDNEY CHECKLAND. *British Public Policy, 1776–1939: An Economic, Social, and Political Perspective*. New York: Cambridge University Press. 1983. Pp. ix, 431. \$54.50.

This useful book by Sydney Checkland covers the century and one-half before the eve of World War II; is divided chronologically into periods ending in 1815, 1851, 1874, 1914, and 1939; and traces the themes of economic transformation and organization, the rise of labor, nationalism in the British Isles, and the development of social policy in health, education, housing, and income redistribution within each period. There is a superb index with five columns of "Statutes," two on "Societies and Associations," and one for "Royal Commissions." A twelve-page list of books is the major scholarly trapping, as there is only one footnote, apart from innumerable cross-references, and one table. It is a book to consult in chapters and parts, perhaps, rather than to read through.

The major theme is the movement toward *laissez faire* in the first half of the nineteenth century and away from it after 1880 or so. Both trends were Darwinian, responding to particular short-term needs, abuses, and defaults. Checkland continuously characterizes policy as "drift," "ad hoc," "pragmatic," "inadvertent," "extempore," "variegated," "piecemeal," "lacking governing principle," and "undefined." Gladstone was a minimalist, to be sure, but most governments after 1874, and especially unions, businesses, and reformers, were moved less by ideas than by conditions. The theme of the book is said to be the conflict between the scientific view of society and the moral duty of government to the poor, but it is hard to find much of the former.

This reviewer was particularly interested in the development of economic policy. It moved first to a policy of free trade, the gold standard, and the annually balanced budget, and then shifted to an industrial, social, and macroeconomic policy conducted by the cabinet with the aid of professional advisers and input from industry and labor. Checkland's heroes, besides Joseph Chamberlain and Lloyd George among politicians, include the "professional reformers," Edwin Chadwick, John Simon, and Sir Charles Trevelyan, who fought the squalor

and misery of the Industrial Revolution, and Ernest Bevin and Sir Walter Citrine, who developed macroeconomic policy for Labour. He gives the city too much credit, perhaps, for pulling together in the crisis of 1890, but is properly disdainful of its insistence on parity for the pound in 1925. Both industry and finance are exonerated from the charge of controlling government, the city being weak in analytical power. Policy, as Checkland sees it, is not the result of conscious decision, but of ineluctable forces, into the clash of which the state is bound to be drawn. It is too bad, in light of this view, that the book stops short of the Thatcher period in which pragmatism is giving way to a return to ideology.

CHARLES P. KINDLEBERGER
Massachusetts Institute of Technology

RALPH MILIBAND. *Capitalist Democracy in Britain*. New York: Oxford University Press. 1982. Pp. 165. \$19.95.

Readers acquainted with Ralph Miliband's *Parliamentary Socialism* (1961) and *The State in Capitalist Society* (1968) will sense that this short book is an attempt to apply his thesis to the past dozen years. Miliband's thesis is that, by embracing wholeheartedly the parliamentary representative system and all it implies, the Labour party assured the failure of real socialism in twentieth-century Britain. The advances toward democracy of the past two centuries, while to a small extent genuine, have been manipulated subtly by the establishment to contain conflict and prevent further democratization of power relationships. Assisting in the process of containment are the civil servants, intellectuals, the media, and, paradoxically, the trade unions, all forces whose primary impulse is to maintain political stability and social peace. Miliband aims to show that "it is a crucial concern of those who run the state and other institutions of power to achieve the containment and reduction of popular pressure. This is not only produced by deliberate management; it is also generated by habits, traditions, and constraints that make for inertia and acceptance rather than for pressure and conflict" (pp. 1-2).

In intention, the book owes as much to Antonio Gramsci as it does to Marx. In execution, it shows neither particularly strong attention to economic change nor says much about issues of cultural hegemony. In focus, its real progenitor is Harold Laski. The perpetuation of capitalist (that is, limited, unequal) democracy in Britain is attributed not so much to the straight economic power of capitalism as to the inability of "moderate" Labour party leaders when in power to advance the cause of socialism. Much of the political skill of these leaders,

and their allies in the big unions, has been devoted to resisting left-wing activists and to working within the elitist parliamentary practices created by economic conservatives. Miliband sees little change in the past dozen years, despite leftward shifts in the Labour party and unions or the industrial action taken against the Edward Heath and Margaret Thatcher versions of conservatism. The book ends on a rather inconclusive but decidedly pessimistic note concerning prospects for socialism and democracy in Britain.

Miliband's treatment of Labour history down to the 1950s, although compressed, is not unsound. Notwithstanding the inclusion of material from the current scene, the work is curiously dated and is redolent of analyses of the heyday of the industrial state. Social science evidence does not give much credence to the assumption that, if fully articulated, "popular pressure" would insist on more socialism and less Parliament. Miliband's extended essay works well as a fair-minded testament of a socialist in the tradition of Laski, G. D. H. Cole, and the young Aneurin Bevan, looking back more in sorrow than in anger. It might well be read in conjunction with another, and fresher, work of the same genre but from a very different perspective, Samuel Beer's *Britain against Itself* (1982).

JAMES B. CHRISTOPH
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DENIS JUDD. *Lord Reading: Rufus Isaacs, First Marquess of Reading, Lord Chief Justice, and Viceroy of India, 1860-1935*. London: Weidenfeld and Nicolson. 1982. Pp. x, 316. £15.00.

As Denis Judd's biography shows, Rufus Isaacs, first Marquess of Reading, was one of the lesser ornaments of the Liberal party in the days of Asquith and Lloyd George, but he was also one of the most able and attractive. His spectacular rise from solid but undistinguished beginnings as the son of a Jewish wholesale fruit merchant included a rakish youth. At sixteen, he sailed as ship's boy on a freighter to Brazil. At nineteen, he lied about his age on an application for membership in the stock exchange. Five years later, he was expelled from the exchange when he could not pay his debts. He was twenty-five when he began to prepare for the bar. Ten years later, he was a queen's counsel and one of the greatest barristers of his day.

The scandal of being "hammered" on the stock exchange—and of having lied when joining it—was only an omen of the misjudgment that landed him in the Marconi scandal. It is a tribute to his good luck, good friends, and good management that he

was able to surmount that crisis and go on to greater triumphs.

His formidable skill as a barrister, his enormous capacity for hard work, and his personal charm and wit gave him entree into high political circles, first as attorney general, then as lord chief justice. It is, however, as the friend of Lloyd George, who was also involved in the Marconi affair, that he is chiefly remembered. This is unfortunate, because it overshadows his success as a wartime administrator and his skillful handling of the difficult job of representing the United Kingdom in the United States, first as a negotiator of major loans in 1915, then as ambassador in 1918–19. Lloyd George kept Reading at his side during the peacemaking process to take advantage of the good working relationship he had forged with President Wilson and Colonel House.

He also deserves to be remembered as the Viceroy of India who conscientiously administered the newly enacted Montagu-Chelmsford reforms in the highly charged aftermath of the Amritsar massacre and the passage of the Rowlatt Acts. He was the first Viceroy who had to deal with Gandhi and the forces he unleashed. Reading was largely successful here, too, as long as his term of office lasted, although his efforts to flesh out the ideals of his friend Edwin Montagu were partly frustrated by diehards in the home government, the Indian civil service, and the Indian army, and his efforts to keep the peace while taking account of British foreign-policy concerns led to a deterioration of Hindu-Muslim relations.

Reading was doubly an outsider as a tradesman's son and as a Jew. Perhaps that endeared him to that other outsider, Lloyd George. His success was not only a personal triumph, but tells us something about the Liberal party and British society of that day. Reading's progress, through his last hurrah as foreign secretary in MacDonald's National Government in 1931, is in some sense a paradigm of Liberal aspirations, achievements, and disintegration as a party.

Judd's sympathetic biography adds nothing original to our knowledge of Reading. Like his other works, it sums up current scholarship in a convenient and attractive form.

BERNARD GAINER
Silver Springs, Maryland

R. J. MOORE, *Escape from Empire: The Attlee Government and the Indian Problem*. New York: Clarendon Press of Oxford University Press. 1983. Pp. x, 376. \$39.95.

This densely packed volume completes R. J. Moore's trilogy on British policy and the end of empire in India, which began with *The Crisis of*

Indian Unity, 1917–40 (1974) and continued with *Churchill, Cripps, and India, 1939–45* (1979). Like its predecessors, this is not so much a forester's book as a logger's: we see a great many trees—too many perhaps to get a sharp picture of the wood as a whole. Moore's response would doubtless be that the story is immensely complicated and that it is necessary to trace it day by day, plan by plan, revision by revision. If that is so, Moore has succeeded, for he has given us a minutely detailed account. In doing so, however, emphasis suffers, and I was sometimes left wondering which events Moore considered important and which he did not. Indeed, teachers might hope that Moore will produce a condensation of the eight-hundred-fifty-page trilogy that will distill the essence for students and nonspecialists.

In the present volume, Moore might have aided his readers in two relatively simple ways. A chronology of events in table form would have allowed readers to keep complicated sequences straight in their minds. Similarly, brief biographical notes would have helped to establish the distinction between, for example, A. V. Alexander (the First Lord of the Admiralty) and Horace Alexander (the Quaker), the three Menons who figure in the book (V. K. Krishna Menon, V. P. Menon, and K. P. S. Menon), and a host of other figures who march through its pages as disembodied names. If they are important enough to be named at all, surely we need to know why.

The book is organized into five chronological chapters. The first, "The Burden of Disengagement," describes the problem the new Labour government inherited in July 1945—a standoff between the Indian National Congress and the Muslim League over the shape of a constitution for an independent India. Chapter 2, "Plan Union," details the Labour government's attempt to break the impasse: the proposal of the Cabinet Mission (Sir Stafford Cripps, A. V. Alexander, and Lord Pethick-Lawrence) for a three-tier federation, which won half-hearted acceptance in June 1946. Chapter 3, "The Challenge of Communalism," outlines the collapse of that fragile agreement in July, the withering of government authority, and the spread of communal animosity throughout India. Chapter 4, "Plan Partition," recounts how a new Viceroy, Lord Louis Mountbatten, quickly discarded the Cabinet Mission Plan and worked toward a quick partition of British India into the two Dominions of India and Pakistan. The final chapter, "The Post-Imperial Order," discusses the difficulties posed by the princely states and the powerful but dispersed Sikh population, and the consequent legacy of bitterness between the two new countries.

Moore's evaluations of the major British figures are perhaps the most interesting parts of the book.

He argues for "major revisions" to the accepted wisdom about the roles of Attlee and Cripps in extricating the British from India (p. vii). Indeed, his sympathy for Cripps (pp. 69–74, 348–49) makes Moore an obvious candidate to undertake the major biography that Cripps deserves. Though he admires Wavell, "a soldier cast in the heroic mould" and notable for "perseverance, courage and nobility" (p. 213), Moore praises Attlee for his boldness in adopting a "fresh approach" late in 1946 and his change of Viceroys. And though he considers Mountbatten's role "has probably been exaggerated," he agrees that Attlee's choice of Mountbatten was, as Attlee himself described it, "an inspiration" (p. 347). Regarding Winston Churchill, Moore is scathing, pointing to his "treachery" (p. 344) in destroying Cripps's negotiations in India in 1942. He endorses the vivid image of Lord Linlithgow, Viceroy from 1935–43, who stated that, in his ideas about India, Churchill was always to be seen "hanging, hairy, from a branch" (p. 345).

In *Churchill, Cripps, and India, 1939–45*, Moore argued that it was essential to have a detailed narrative of "*haute politique*," as well as "history from below," if we were to gain a full understanding of the processes that brought independence to India and Pakistan (pp. vii–viii). The third volume of his trilogy completes the huge task he set himself. To be sure, the big questions (for example, "when did partition become—and what made it—inevitable?") remain unresolved. But for those who seek to wrestle with them, much of the high-level detail they will need to know is now readily available in Moore's three books.

ROBIN JEFFREY
La Trobe University

GEOFFREY ALDERMAN. *The Jewish Community in British Politics*. New York: Clarendon Press of Oxford University Press. 1983. Pp. ix, 218. \$34.95.

Geoffrey Alderman's book certainly makes good its claim that it fills a scholarly lacuna. Essentially a study of the Jewish electorate since emancipation (admission to Parliament) in 1858 to the election of 1979, the work documents political trends and displacements within the Jewish community and shows that they closely paralleled those of the rest of the political population. English rather than British, however, would have been more accurate in the title: Scotland and Wales are not part of the inquiry, and just enough is said about Ireland to make one regret that Alderman passed over the Celtic realms. The book really begins with the second chapter, for the first is a summary of earlier works. Seemingly innocent of studies done by Lewis Namier, Peter Pares, and other scholars, Alderman commits a few

minor errors in characterizing eighteenth-century politics. Many of Alderman's findings are based on his electoral research and interviews.

Alderman's exposition partly confirms the proverbial claim of the Board of Deputies that Jewish political behavior is indistinguishable from that of the British electorate as a whole, that Jews vote not as Jews but as Britons. His best chapters, however, are those in which he demonstrates that there have always been issues, for example, emancipation, immigration and alien legislation before 1914, Zionism between the wars, and Israel since 1948, to which English Jews, *pace* their leaders, responded in specifically Jewish ways. Alderman welcomes such independence and conducts a running battle with the leadership, past and present, for being "faint-hearted" (p. 21). His retrospective condemnations add zest, but are fundamentally unhistorical. He is, however, especially penetrating on the post-1945 "Return to the Right," when the Jewish vote emerged as something worth contending for; it is symbolized by the fact that in Margaret Thatcher's constituency of Finchley the Jewish vote was "crucial" and that she was the first leader of a major party to sit for such a constituency (p. 146).

Alderman's judgment on Gladstone is extreme, but his is not the last word on the Liberal chief who, no doubt, was too much the devout Christian to be capable of genuine empathy toward the Jews. Mention is made of Churchill, principally as the wrecking of the severe Alien and Immigration Bill of 1904, and Lloyd George, largely for his "particularly crude way" of smiting Lord Rothschild (p. 84). Such episodes could have been more illuminatingly interpreted in the context of "the strange [or natural] death of liberal England." Alderman had not bothered to consult the important reports of the two select committees (1888) or of the royal commission (1903) on immigration, but was content to rely on three-page summaries by V. D. Lipman!

An incisive chapter, entitled "Love Affair with the Left," deals largely with the Labour party, but also briefly discusses the "infatuation" with communism. There is nothing here on rescue politics between 1939 and 1945, a desideratum since Bernard Wasserstein's book confines itself to the government's role.

Some defects of Alderman's book have been pointed out here, and others might be enumerated. But they are all minor in a work that breaks new ground, does it with verve, and ought to be suitably incorporated in future studies of British politics. Alderman has obviated any excuse to leave out the "Jewish dimension," a characteristic sin of historiographical omission made in works on British politics.

FREDERICK M. SCHWEITZER
Manhattan College

JOSEPH GORNY. *The British Labour Movement and Zionism, 1917–1948*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1983. Pp. xvi, 251. \$30.00.

Although the future of Palestine was, for the British Labour movement, only a marginal interest during most of the years covered by Joseph Gorny's study, his examination of the development of Labour's views on the question fills a distinct need. When Labour governments faced crises in Palestine in 1929–31 and again after World War II, their responses were conditioned to some extent by attitudes and commitments that had evolved over the years. Until the publication of this work, it had been difficult to determine the background with which Labour leaders approached the problems of the mandate.

If Palestine was a secondary concern for British Labourites, the views of British politicians were of immense importance to Zionist leaders. Gorny has made effective use of the correspondence, reports, and minutes of meetings in various Israeli archives in which Zionist contacts with British Labour leaders are reported in full. These, in conjunction with more familiar sources such as the private papers of British Labour leaders and official Labour party and cabinet documents, provide a solid basis for the study.

Gorny finds that the Zionist cause evoked a generally sympathetic response among Labourites, who were attracted by its socialist flavor. Although Labour, concerned about Arab opposition to Zionist ambitions, was at first cautious about commitment to a Jewish national home in Palestine, support for the cause gradually increased in Labour circles during the 1920s. In the 1930s, Labourites, moved by the plight of Jewish refugees from Central Europe, pressed the Conservative government to increase Jewish immigration quotas in Palestine despite Arab opposition.

Labour's sympathy for Zionism was put to severe tests during the party's years in office. Faced with Arab riots in 1929, the colonial secretary, Lord Passfield (Sidney Webb), responded with a white paper that constituted, in the Zionist view, a retreat from the party's earlier pro-Zionist stance. In that instance, assiduous pressure from Zionists led to a considerable softening of the government's proposed curbs on Jewish immigration and land purchases. In the years 1945–48 the Zionists were much less successful in gaining acceptance of their views when the Labour government, spurning the solution of partition, handed the mandate over to the United Nations.

Gorny's conclusions are usually reasonable and well balanced. It is curious, however, that in his extensive speculations on the reasons for what he

perceives as Passfield's anti-Zionism, he fails to consider the possible influence the pro-Arab views of Beatrice Webb's nephew, the former Palestinian official, Colonel Richard Meinertzhagen, may have exerted on the colonial secretary. One might also question his rather harsh evaluation of Clement Attlee and Ernest Bevin as mere bumblerers in their handling of Palestine. Gorny's suggestion that "political daring, social vision and moral sensitivity" (p. 201) could have solved the tangled problems of the Middle East seems far too optimistic.

For the most part, however, this work provides a judicious treatment of the relations between the British Labour movement and Zionism that will be helpful to students of both.

CATHERINE ANN CLINE

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WILLIAM J. SCHOENL. *The Intellectual Crisis in English Catholicism: Liberal Catholics, Modernists, and the Vatican in the Late Nineteenth and Early Twentieth Centuries*. (Modern British History.) New York: Garland. 1982. Pp. xii, 341. \$45.00.

William J. Schoenl has written a useful and informative volume on a group of Edwardian Roman Catholics usually lumped together under the term "liberal." His protagonists are Baron Friedrich von Hügel, the religious philosopher; Wilfred Ward, the editor of the *Dublin Review*; George Tyrrell, the Jesuit biblical critic; Edmund Bishop, the historian of liturgy; and Francis Aidan Gasquet, the Benedictine historian of the English Reformation. The book concentrates on the first three writers. In their various fields they attempted to reconcile the doctrines of the Roman Catholic Church with the discoveries and theories of modern science and history. Traditionally, they have been regarded as either exponents of or sympathizers with Catholic Modernism. They encountered the double problem of being regarded as suspect by other English intellectuals because of their Catholicism and as difficult by Roman Catholic authorities because of their liberalism.

Schoenl recounts the familiar story of the tensions that these Roman Catholics experienced with the church and their ultimate failure to win over the Vatican authorities. He adds two original dimensions to that account. First, he clearly establishes a line of perceived continuity between his group and the mid-Victorian liberal English Catholics, such as Lord Acton and John Henry Newman. In this part of the study Schoenl provides a particularly helpful discussion of the battle of St. George Jackson Mivart, the biologist, with church authorities. Second, Schoenl firmly establishes major differences of temperament and theological outlook among liberal

Catholics. In that respect, his "group" eventually dissolves into a set of really quite independent and not always even mutually sympathetic individuals. Gasquet and Bishop are willing to protest privately against the Vatican but accommodate themselves publicly. Ward presses for the freedom of a Roman Catholic journalist, but ultimately recedes into literary criticism. Von Hügel urges others onward but is determined to remain in the church. Tyrrell emerges as the only real theological and ecclesiastical rebel—the only genuine Modernist—and dies outside the church. Schoenl might have added another dimension to the book by a more thorough consideration of what these divisions meant politically for the fate of liberal Catholicism.

Three major problems appear in this study. In arguing for the links with the Victorian liberal Catholics, Schoenl fails to note that the Edwardians themselves provided the major interpretation of those predecessors. Wilfred Ward himself wrote the great biographies of Cardinal Wiseman, Cardinal Newman, and W. G. Ward, his father. More attention should have been devoted to a consideration of the Edwardian use of Newman's concept of development. Was the Edwardian meaning the same as Newman's? Moreover, there is need for much more biographical detail. Why did these writers become *liberal* Catholics? What was the character of their social and theological development? To what extent and in what manner did the English intellectual and religious setting contribute to their activities? Finally, their opponents in both the English Roman Catholic Church and the Vatican require more systematic investigation. In the pages of this volume, as in most other treatments of the subject, the victorious ecclesiastical authorities emerge as shadowy figures who slay the children of light. Such a portrayal may serve the demands of intellectual justice but not of historical scholarship.

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A. J. ENGEL. *From Clergyman to Don: The Rise of the Academic Profession in Nineteenth-Century Oxford*. New York: Clarendon Press of Oxford University Press. 1983. Pp. xi, 302. \$49.50.

In 1850 Oxford University was essentially a clerical society; its unmarried fellows, whether engaged in teaching or holding their fellowships as prizes, looked forward to careers as parsons. By 1900 the college fellowship, no longer bound by celibacy or Anglicanism, had become a teaching career in itself: "The traditional clerical Oxford had been destroyed to make way for a new secular profession." This is the major theme of A. J. Engel's book, originally a Princeton dissertation. It is a theme that invites comparison with a similar work on Cambridge,

Sheldon Rothblatt's *Revolution of the Dons* (1968). In Oxford, as portrayed by Engel, it was truly the dons, narrowly defined as college tutors and lecturers, who were the prime beneficiaries of the revolution, whereas in Rothblatt's depiction of Cambridge, the university, with its professors and scientific laboratories, shared in the benefits. Engel's work is more narrowly (and tightly) focused on the development of the teaching profession as such, in contrast to the wider educational and societal concerns of Rothblatt or of W. R. Ward in his general study, *Victorian Oxford* (1965).

Engel easily succeeds in demonstrating his unremarkable thesis that "central to the intellectual conflicts and institutional changes in nineteenth-century Oxford was the question whether the University ought to provide a career for academics and, if so, what sort of career it should be in terms of functions, status, and duties." By 1881, as a result of royal commissions and legislation, the decision in favor of the academic career had been made. But a funny thing happened on the way to academia. While collegiate tuition was provided for, little was done to endow research or to encourage science, nor was adequate provision made for the progression of college lecturers to higher ranks. The dons became the prisoners of their own revolution, trapped (albeit genteelly and uxoriously) in their college tutorships. This is Engel's secondary but more original theme.

The first part of Engel's book, demonstrating his primary thesis and going up to the royal commission of 1877–81, is a work of social history: the making of a profession, comparable to the other Victorian professions. The second part, through 1914, is dominated by the economic factor and comes perilously close to economic determinism. The confining nature of the tutorial career and the limited development of science and research are attributed primarily to the agricultural depression, which deprived both the colleges and the university of resources that might have made further development possible and, thereby, might have exacerbated conflicts between generations and between the "two cultures." This economic explanation, though neither unqualified nor unsound, has already been controverted and will remain controversial. Surely something should be attributed to the "ethos" of the place. Engel's social-science approach may be deficient as regards those nuances that, in academic politics, are often more important than economic realities. Nonetheless, his book, although sometimes stolid, is generally solid and makes an important contribution to the history of the professions and the universities.

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JOHN A. BLYTH. *English University Adult Education, 1908–1958: The Unique Tradition*. Dover, N.H.: Manchester University Press. 1983. Pp. xii, 363. \$20.00.

The unique tradition of John A. Blyth's subtitle is the attempt, begun by R. H. Tawney in 1908 under the auspices of the Workers' Educational Association (WEA), to offer the British working class a rigorous education of university standard in three years of evening classes, with all the Oxbridge devices of personal tutoring, group discussion, and essay writing. It was a great ideal, inspired by the Balliol College tradition of service by the privileged intellectual elite to the deprived and underprivileged poor. It was admirably suited to a period in which class barriers were higher than before or since and in which intelligent working men and women thirsting for knowledge had little chance of getting even secondary education, still less of utilizing their undoubted talents at the university. Only an age guilt-ridden, on the one hand, with the "consciousness of sin" that Beatrice Webb diagnosed in the middle class, and, on the other, and despite the new Labour party, deferential to learning and the effortless superiority of young, high-minded, but inexperienced Oxford men, could have invented for the working class a "liberal" adult education "for its own sake" with no qualification or career prospect at the end of it. It was a well-meaning but nonetheless patronizing offering that was all the more surprising for being accepted and even loved by the proud and independent British worker.

What is most surprising is not that it failed in the end but that it lasted so long, beyond the fifty years chronicled in this book. It was attacked from the beginning by the left-wing working-class students of the Plebs League and the National Council of Labour Colleges, which get no mention in Blyth's book. His main concerns are the long competition for students between the WEA and the university extramural departments and the decline of committed working-class students and their replacement by leisure-oriented teachers, clerks, and housewives of the middle class. The great debate, which was not between the universities and the WEA but split them both, came to a head in the controversy in the 1950s, famous to those of us involved, between two university extramural directors, Sidney Raybould of Leeds, who stood by the twin ideals of university standards and working-class students, and Robert Peers of Nottingham, who held that such ideals were out of date in the postwar Britain of "secondary education for all," with the largest proportion of working-class university students in the West. Neither of them imagined that there was a vast adult reservoir of talent in all classes waiting to be tapped by real university education with degrees at the end

of it, as the Open University was to prove when it opened its doors in 1969.

This book, the product of a competent PhD, looks almost as if it has been photocopied from the thesis. It chronicles a unique episode in the class-ridden story of English education, but for this reviewer lacks the imagination to realize its full significance.

HAROLD PERKIN
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JOHN K. WALTON. *The English Seaside Resort: A Social History, 1750–1914*. Leicester: Leicester University Press or St. Martin's Press, New York. 1983. Pp. xii, 265. \$37.50.

John K. Walton of the University of Lancaster is already well known among English social historians for his perceptive and original essays on the development of that most characteristic institution of nineteenth-century England, the seaside resort. In *The English Seaside Resort: A Social History, 1750–1914*, he has ranged comprehensively across the whole subject to produce a most interesting and well-written general history that will long remain the field's standard work. Envisioned as a contribution to a "study [of] wider groupings and 'families' of English towns," the book's central themes are the "enormous variety of resort experiences" and the seaside's "influence on lifestyles, attitudes, and consumption patterns." Although the emphasis of the book is on the period 1860–1914, seaside resorts, like Brighton, developed earlier. Inland spas, like Bath, are discussed in a sophisticated and salient way. Among the many details to emerge, possibly the most striking is the sheer number and variety of seaside resort towns and villages. By 1911, according to Walton's count, there were 145 such resorts north, south, east, and west, ranging in size from Brighton, Bournemouth, Southend, and Hastings, each with more than sixty thousand inhabitants (Brighton alone had over one hundred thirty thousand) to Charmouth, Studland, Alnmouth, and Bonchurch, whose combined population was just over two thousand.

To a surprising extent, the development and maturation of England's resorts followed the class and sectional lines of wider society. If one were to name two locales that most connoted "middle class" and "working class" (or "Tory" and "Labour") in modern Britain, Bournemouth and Blackpool (respectively) would do quite as well as, say, Kensington and Oldham—indeed, a lot better. Walton outlines the reasons for this in a cogent way. For instance, Walton charts the (usually unsuccessful) efforts by town corporations and others to separate the social classes at the seaside, a continuously losing battle that compelled middle-class vacationers to rub

shoulders with the hawkers, buskers, minstrel men, stall keepers, and catchpenny amusements so characteristic of the English seaside resort. Despite this, to a surprising extent privacy within the family circle was the aim and goal of nearly all those on holiday, with the boardinghouse (discussed by Walton in an excellent section) and the apartment hotel prevailing over holiday-camp-type common boarding and dining facilities nearly everywhere.

Walton has provided illumination and illustration nearly everywhere in a work remarkable for its command of the relevant local and national sources, and one can find few points of criticism worth making. At a general level—although he is obviously aware of the distinction—one wonders if a sufficient difference has been drawn between the true holiday resort found more often in the north and the seaside resorts of the south and west of England that were dominated less by the holiday trade than by a permanent population of the retired, semiretired, and commuting middle classes. A villa in Bournemouth or Eastbourne was, after all, a substitute for one in Bayswater or Richmond, or for twenty acres in a shire, and represented a substantial shift of values by the middle class. Although Walton touches briefly on the matter, one would have liked a discussion of contemporary resorts elsewhere—Newport, Coney Island, and Atlantic City in America, for example—that, like so much else in popular culture, seemed to appear in parallel but distinctive fashion throughout the advanced world between 1865 and 1900.

Apart from its historical qualities, *The English Seaside Resort* is an extremely well-produced monograph with amusing and relevant illustrations and an attractive format that might be studied with profit by many an academic publisher. Despite the variety of resort development found by Walton, the sectional, class, ideological, and intellectual preconceptions brought by Englishmen of every description to their seaside resorts were merely typical of larger British society. The Englishman was never on holiday.

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JOHN J. CERULLO. *The Secularization of the Soul: Psychological Research in Modern Britain*. Philadelphia: Institute for the Study of Human Issues. 1982. Pp. xvi, 194.

Psychical research enjoyed a respectable popularity in late Victorian and Edwardian England. Recent scholarship has sought to place this curious phenomenon more securely in the context of the confused intellectual history of the period and to illus-

trate its relationship with changing currents in philosophy, science, and religion.

John J. Cerullo's principal thesis is that psychical researchers "recast the concept of the soul into a form more palatable to scientists, [and] came up with a formulation I call 'the secular soul'" (p. xii). Cerullo concentrates on the psychical research activities of only a few men, especially Frederick W. H. Myers, Henry Sidgwick, and Edmund Gurney. Notable is the author's summary assessment of the life and work of Myers, whose posthumous *Human Personality and Its Survival of Bodily Death* (1903) is described as "one of the key documents in modern European intellectual history, just as Myers himself may be a much more pivotal figure than he is now acknowledged to be" (p. 101). Surely it is remarkable that Myers was speculating and writing about "subliminal consciousness" and "multiplex personality" even before Freud began to formulate his ideas on the unconscious. Significantly, it was Myers who introduced Freud to the English reading public as early as 1893. Whether Myers's "subliminal selfhood" was the "secular soul" of which Cerullo speaks, however, is another matter. Certainly the evidence cited does not substantiate the assertion that the skeptical Sidgwick was seizing the opportunity through psychical research "to actually secularize the soul" (p. 43). There is nothing in Sidgwick's writings to substantiate such an ascription of motivation.

A more serious flaw in this book is the near total omission of reference to the physicist Sir Oliver Lodge, one of the most prominent figures in the Society for Psychical Research and Myers's closest friend. The omission is a wonderment in view of Cerullo's comment about "the psychical researcher's audacious attempt to engrave a religious commitment onto a scientific world view" (p. 105). Far from attempting to secularize the soul, Lodge was keenly aware of the potential support that psychical research could give to religion. He was ever conscious that if empirical proof of the reality of the spiritual world and of the survival of consciousness were forthcoming, there would be momentous implications for the understanding of traditional Christianity. Through a series of articles and books this hope became a confirmed belief for Lodge.

Instead of providing us with information about Lodge and the many other notable psychical researchers in a chapter entitled "The Constituency for Psychical Research," Cerullo gives us several useless pages on a few completely unmemorable men. Scholars will note several omissions in the bibliography, not the least of which is the absence of Lodge's name. Scholars also will wonder how the author could have done research in the archives of the Society for Psychical Research in London and not made use of the Lodge and Myers papers there.

Nor is there any reference to the Sidgwick and Myers papers at Trinity College, Cambridge.

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DAVID ARMSTRONG. *Political Anatomy of the Body: Medical Knowledge in Britain in the Twentieth Century*. New York: Cambridge University Press. 1983. Pp. xii, 146. \$29.95.

Michel Foucault's impressionistic conclusions about the disciplinary effect of medical practice in the late eighteenth century inform this study of twentieth-century medicine as an expansionist, specialized apparatus of social power and control. David Armstrong, a practicing doctor who became a sociologist of medicine, begins with Foucault's "Panopticon" model, which constructed the passive individual body through observation, analysis, and documentation. In the dispensary, with its invention of a relative and subjective body, Armstrong finds the twentieth-century model for extended surveillance and control within the wider community. The dispensary changed the focus of the "medical gaze" from individuals within walled hospitals to the monitoring of the whole social body *qua* patient.

First in tuberculosis and then in venereal disease and pediatrics, medicine manipulated and transformed individuals by means of dossiers, medical exams, school reports, and food distribution. The justification for increasingly intrusive practice was that social health required supervision. In the invention of "neuroses" as a marginal form of mental disorder suffered by everyone, general practitioners enlarged their sphere to all the aspects of social life. The result was, Armstrong maintains, a new, unitary, medical practice that obliterated distinctions between mind and body, normal and abnormal, health and disease. Instead there was a constant surveillance of children and adults in families, neighborhoods, schools, and places of work. Armstrong contends further that these medical observations led to reconstructed individual personalities within reordered social relations. Social medicine, accelerated by the military's successful medical surveys during the war, emerged as a separate discipline in 1943 in the Institute of Social Medicine at Oxford. This new field searched for social pathology by studying people in groups. With the development of the detailed medicosocial survey in the 1920s, the sick and well were together enmeshed in an intricate mechanism of observation. The survey spawned the new medical specialties of pediatrics, psychiatry, general practice, and geriatrics. From the late 1940s, a comprehensive "community gaze" imposed a new diagram of power on society. While the dissecting gaze of the nineteenth century fo-

cused in fear on Frankenstein's monster, Armstrong suggests that the twentieth-century "community" gaze leads to the greater terrors of Huxley's and Orwell's controlled societies.

Armstrong believes that the specialization of medicine in our century is not due either to the development of a corpus of knowledge or to ambitious professional goals. Instead, he sees the growth of medicine as a product of the subtle, pervasive relationship between perceptions of the body and powerful institutions embedded in the welfare state. In all the sciences of man, and especially in the relation between sociology and medicine, Armstrong finds components of the larger structure of power that extends its discipline over even the ordinary and trivial aspects of social life.

Armstrong's evidence is derived essentially from medical texts, journals, reports, and teaching manuals. But the social, economic, and political evidence apart from medicine demonstrates unequivocally how thoroughly administrative muddle and the vigorous recalcitrance of groups and individuals to administration have defeated social control. This brief, well-written essay reveals both the weaknesses and the strengths of sociological analyses of historical events: it is too narrow in its focus and its guiding hypotheses, but it is imaginative and provocative.

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JONATHAN SCHNEER. *Ben Tillett: Portrait of a Labour Leader*. (The Working Class in European History.) Urbana: University of Illinois Press. 1982. Pp. 241. \$23.95.

As Jonathan Schneer's book demonstrates, Ben Tillett was a complex, mercurial character at any time. In the unsuccessful Bristol dock strike of 1892, he "had begun . . . by counselling prudence," but soon "demanded of his willing followers that they swear an insurrectionary oath," and then "concluded by disclaiming in court, against all the evidence, any revolutionary intent" (pp. 93-94). There were also wide oscillations in his outlook over the course of his career. He changed from a militant new unionist to an advocate of compulsory arbitration, then to a revolutionary syndicalist, and finally, in wartime and after, to an "industrial conservative" (p. 197). In politics, the fiery socialism of the early nineties was followed by flirtations with Liberalism, which in turn were followed by an onslaught on Labour party leaders in 1908, and in 1914 by extreme anti-German chauvinism.

Immediately after a crisis in his affairs, Tillett's health would break. "He would be out of commission for months at a time, his illnesses real but

accompanied by . . . depression amounting on more than one occasion to complete nervous breakdown" (p. 114). Cures varied from voyages to Australia to "whisky mixed with raw eggs" (p. 121). One shift that was apparently never reversed was from teetotaler to "the scourge of 'the Temperance bleating martyrs'" (p. 135).

By researching and recounting these complexities, Schneer has made a welcome contribution to labor history. He goes on to discover a thread of consistency through them. "The war . . . revealed Tillet's true colours." Previously Tillet had camouflaged "his objective role as a trade union negotiator . . . of compromises with violent rhetoric" (p. 197). During the war he became, and remained, "an advocate of industrial peace" and "one of the conservative 'Old Guard'" who warned "younger labor leaders . . . against the bolsheviks and hotheads of the movement" (p. 201). Perhaps, but a man's true colors are not necessarily those he flies in his dotage.

It is possible to offer a more functional explanation of the erratic course of Tillet's opinions. To organize casually employed dockers requires, initially, a stirring victory to commit them to trade unionism followed by a firm and lasting agreement with the employers to allow the union to impose a union shop. Otherwise, when enthusiasm wanes, the employers will replace trade-union dockers with the nonunionists outside the gates. To achieve a stable agreement with the employers, the union must have a disciplined membership that will respect the agreement. Between 1889 and 1892 Tillet succeeded brilliantly in the first task, but failed in the second. For many years thereafter he thought, mistakenly, that compulsory arbitration might provide the means to success. He reverted to militancy, however, by the summer of 1911 when he and other leaders of waterfront unions got a second chance. The dockers and seamen won another epic victory, but in London they failed to get the kind of agreement they needed and lost out again in the 1912 strike. Elsewhere James Sexton's dockers and Havelock Wilson's sailors and firemen had gotten the kind of agreements they wanted and refused to jeopardize them by coming to the aid of the Londoners in breach of their own undertakings. That is the reason for the failure of the call for a national strike in 1912, which seems to puzzle Schneer (pp. 161–62). Tillet was saved by the wartime and immediate postwar settlements that gave his London dockers what they had failed to win for themselves. Militant leadership was no longer needed.

HUGH CLEGG

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JOHN HELLMAN. *Simone Weil: An Introduction to Her Thought*. Waterloo, Canada: Wilfrid Laurier Univer-

sity Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. viii, 111. \$11.00.

Forty years after her death in England during the Second World War, the personality and writings of Simone Weil exercise an enduring fascination. A dissident Marxist in the early 1930s, she despised communist politicians who knew little of the realities of physical labor. Deeply attracted by Christianity later in the decade, she hated the legalism and bureaucracy of Rome. Her fellow Jews were troubled by her violent repudiation of Judaism. De Gaulle thought she was mad. Many regard her as a saint.

Weil's views, the products of a brilliant but erratic imagination, are scattered in many essays and articles and cannot be grasped easily. They need considerable commentary and elucidation. This study by John Hellman is a noteworthy addition to the corpus of criticism and commentary that we now possess. After a chapter on Weil and her times, he goes on to examine Weil's attitudes toward Marxism and Catholicism and shows the basic consistency of her thought. Despite superficial changes of allegiance, she remained constant in her libertarianism, her commitment to the poor, and her deep spirituality. Even in her Marxist phase, the basically religious nature of her thought was quite apparent: Trotsky thought that she ought to belong to the Salvation Army; her contemporaries referred to her as "the Red Nun."

This study breaks some new ground in underlining the basic consistency of her attitudes, and Hellman is particularly interesting in demonstrating how early in her career Weil came to realize the inadequacy of politics in dealing with the complexities of the human condition. Where the book is not completely satisfactory is that it in no way offers a comprehensive introduction to Weil's thought: much essential biographical information is not provided; her mathematical and philosophical writings are not given much attention; and her best known work, "The Need for Roots," is only mentioned once.

The lack of information about Weil's last years is particularly unfortunate since any interpretation of her achievement is bound to be affected by the answers one gives to important questions that inevitably arise out of an examination of the closing stages of her career. Was "The Need for Roots" truly an original work or was it so heavily influenced by the writings of Gustave Thibon that it really represented only a passing phase in Weil's evolution? Was she completely aware of what she was doing in the last months of her life or was her avowed intention to eat no more than the rations allocated to the people of occupied France a form of self-deception to conceal from herself the fact that

she was in the grip of anorexia nervosa? These are some of the questions that cause Weil's reputation to be the subject of controversy, and Hellman's intelligent book would have been better if they had been answered.

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GERALD D. ANDERSON. *Fascists, Communists, and the National Government: Civil Liberties in Great Britain, 1931–1937*. Columbia: University of Missouri Press. 1983. Pp. 243. \$20.00.

During the 1930s, many countries encountered the challenge of extremist politics that threatened to undermine existing constitutional procedures and usurp the rule of law. For the parliamentary governments of Western Europe the period exposed one of the dilemmas of liberal democracy: to protect traditional freedoms in one direction, an erosion of civil liberties might have to take place in another. The United Kingdom was no exception, for although the swing toward extremism during the age of MacDonald and Baldwin can be exaggerated, the government was on more than one occasion seriously alarmed by the threat posed by communist and fascist organizations. Two major pieces of legislation, the Incitement to Disaffection Act of 1934 and the Public Order Act of 1936, marked attempts by the British government to control extraparlimentary movements of both left and right and to preserve its authority against a possible breakdown of public order. Both acts involved incursions into civil liberties, and their impact became part of a wider political debate in which the government was faced by accusations, particularly from the left, of repression, discrimination, and partiality.

Gerald D. Anderson's book represents the first full-length modern study of the struggle over civil liberties in Britain between the fall of the second Labour government in 1931 and the passing of the Public Order Act in 1936. He examines the background to legislation affecting civil liberties in the period since the Great War and the nature of the communist and fascist movements in Britain, including some of the major turning points in the government's attitude toward them during the 1930s, notably the controversial Black Shirt rally at Olympia in June 1934 and the clashes between police and antifascist demonstrators in the East End of London in October 1936. Anderson concludes with an assessment of the impact of public order legislation on the organizations of the extreme left and right and of the British government's record in regard to civil liberties. Anderson argues that the Public Order Act did have some effect in limiting political violence and that, while some erosion of

civil liberties inevitably took place, there was little excessive abuse of its powers by the state.

This analysis, based as it is on an extensive use of primary sources, is both well documented and judicious. Moreover, Anderson's discussion of the passage of the Incitement to Disaffection Act fills an important gap. It is, however, unfortunate that elsewhere the author is often following an already well-beaten path and has frequently failed to derive advantage from other research. The role of the hunger marches in the late 1920s and early 1930s in sharpening the focus on public-order issues is underplayed and would have benefited from reference to Ralph Hayburn's seminal article on the police and the hunger marchers (1973). Equally strange is the lack of reference to Skidelsky's major biography of Mosley, available since 1975, which explicitly confronts some of the issues raised by this book. Nor is there proper consideration of the wider issues of police discretion in the prosecution of minor public-order offences, such as obstruction, or of the use of informers and special-branch surveillance against extremist groups, both of which have generated literature in articles and theses during the past decade. These limitations apart, however, Anderson has provided a useful contribution to an important aspect of modern British history.

JOHN STEVENSON
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STEWART J. BROWN. *Thomas Chalmers and the Godly Commonwealth in Scotland*. New York: Oxford University Press. 1982. Pp. xviii, 438. \$55.00.

To the Victorians, Thomas Chalmers was the greatest British-Protestant churchman, a man whose reputation and influence spread far beyond the narrow stage of Scotland to England, Europe, and America. For all that, the complexity of his career and character, compounded by a daunting volume of private papers in an abominable hand, has hitherto deterred historians from making any serious, comprehensive modern study of his life. Following an early tradition of hagiography, the approach to Chalmers in recent general works on nineteenth-century history has been critical, if superficial. To some, Chalmers appears as the "evil genius" behind the decline in religious influence in Scottish society; to others, his main legacy has been to perpetuate the naive idea that the poor, because of their moral weakness, are usually to blame for their own poverty. He was at times undeniably arrogant, quarrelsome, and unpleasantly ambitious; an orator whose sermons seemed banal in print; and a champion of the Establishment who, in his native land, presided over its collapse. It has become hard to see why Chalmers

was deeply admired by men as different as Thomas Carlyle, Lord Henry Cockburn, Sir Robert Peel, William Gladstone, George Canning, Jean Baptiste Say, and François Guizot.

Stewart J. Brown's biography, without either disguising his blemishes or being dazzled by his charisma, concentrates on the central core of Chalmers's life work, his sustained attempt to develop a critique of the heartlessness of laissez-faire capitalism by contrasting it with communitarian Christianity. In this regard, Chalmers believed that the church should take the lead in the moral reformation, education, and self-help of industrial society and that the state should throw its weight behind the church in its great task. "Arch-parson Chalmers," as Marx called him, is seen as nearer to Robert Owen than Adam Smith.

Brown, with great skill, traces the development of Chalmers's communitarianism from his childhood days in the close-knit Fife borough of Anstruther, through his development at Kilmany and Glasgow, to his last great push in Edinburgh when, as a dying man already disillusioned by the Free Kirk, he tried to return again to the idea of the slum redeemed by Christian action. The key to Chalmers's failure lay in his inability to convince either the working class or the politicians that his idea was worth supporting. He was able to weave a spell around those of the middle class who were of evangelical disposition, inspire a generation to visit the slums, call forth great deeds of philanthropy, and ignite the social conscience of a nation. He achieved a power base within the Church of Scotland unequalled by any leader since Knox and presided over the demolition of the Moderatism that had dominated the Scotland of his youth. But the working class did not wish to be saved by house-to-house visiting and the government did not regard it as its duty to give money to the Church of Scotland to build new churches in the towns, so the whole ideal was doomed.

The situation was made worse by the strange episode of the Disruption that broke the kirk in two in 1843. Brown's account by implication places the blame fairly and squarely on the treachery of the Whigs, who encouraged the church to reform patronage in the first place, and on the incompetence of the Tories, who failed to produce a compromise about patronage that would have enabled Chalmers to hold the wild men of his party in check. It is an interesting and persuasive exposition, but it glosses over the evident fact that Chalmers made a catastrophic misjudgment concerning the number of who would join him in a free kirk. He found himself leading a rump of 38 percent of the ministers instead of almost all of them. It was not just the timeservers who failed to follow Chalmers, but many excellent men who for various excellent reasons remained behind to rebuild the Church of

Scotland after 1843. This, however, is a small criticism.

This is a brilliant book—lucid, well written, and deeply analytical. No one can read it without gaining new insight into the nineteenth-century mind.

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WILLIAM P. L. THOMSON. *The Little General and the Rousay Crofters: Crisis and Conflict on an Orkney Crofting Estate*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. x, 234. \$37.50.

A most noteworthy feature of the history of the Orkney Islands in the north of Scotland is the dramatic suddenness with which an agricultural revolution transformed the local landscape, farming methods, property relations, and indigenous culture in the mid-nineteenth century. Within the lifetime of a single individual, Orkney ceased being a region of agricultural neglect and meager yields and emerged to the forefront of modernization among Scottish counties. In 1883, however, the local economy succumbed to the effect of the depression and prosperity ceased.

This book by William P. L. Thomson examines the social history of the modernization process on a single estate located on the island of Rousay, principally between 1840 and 1890. During this period virtually all of Rousay became the property of General Frederick William Traill-Burroughs, who, after thirteen years of service in India, settled permanently on his estate in 1870. Burroughs was a curious figure. He was a product of a rootless family of empire builders and did not fit in well with his tenants or his fellow lairds. The pervasive paternalism that he imposed, combined with flagrant rack-renting, confirms Thomson's judgment that the years in India were "devoid of any experience which fitted him to guide the destiny of a community of Orkney farmers" (p. 15).

Thomson has chosen well in focusing on Burroughs and Rousay: the "Little General" was a leader among his fellow lairds in making improvements while, simultaneously, the Rousay crofters were exemplary in their resistance to his rule during the years of conflict, 1883–89, following the visit of the Napier Commission. The author offers an informed, well-researched study that asks and, for the most part, successfully answers the more important questions that emerge from studies of conflict, namely, the development of oppositional leadership and ideology, conflicting notions of property rights within identical social contexts, the shifting balance of social forces, the role of external influences, and the enduring impact of the conflict on the social

landscape. In short, the reader witnesses the evaporation of any sense of community as Rousay society develops deep fissures along lines of social class, although Thomson suggests that concepts of class might have less operational validity in an island society bound by intimate ties of kinship.

While Thomson's overall picture of Burroughs is a damning one, it is not an exercise in demonology. He shows the general to have been a man of principle and courage, although he stresses that these impulses take an atavistic form ill-suited to the situation at hand. Similarly, in his depiction of the crofting community in Rousay, Thomson avoids the romantic antiquarianism of so much recent literature. Stressing that crofters were a new type of peasantry unique to Scotland and "not members of an ancient class fighting for survival in an era of change" (p. 39), he utilizes social class as a historically specific, evolving formation.

Thomson's excellent book is the best study to date of the agricultural revolution in Orkney, but it has a larger relevance for those seeking to broaden their understanding of rural society in general and rural conflict specifically. My only quibble is that the study ends rather abruptly with Burroughs's death in 1905. It would be interesting to learn of the ultimate fate of the estate, the Rousay crofters, and Orkney society. Perhaps that calls for another book.

GILBERT SCHRANK
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DONAL A. KERR. *Peel, Priests, and Politics: Sir Robert Peel's Administration and the Roman Catholic Church in Ireland, 1841-1846*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. viii, 399. \$49.50.

This volume is an excellent example of a genre that present-day financial pressures are causing to disappear: the Continental-style doctoral thesis that is published not as a book, but as a long, almost infinitely detailed research report that establishes the ability of the author to conduct a meticulous scholarly investigation over a considerable span of years. The publication of this PhD thesis in the "Oxford Historical Monographs" series makes conveniently available a dissertation that otherwise would have remained obscure and difficult to obtain.

This is essentially a history of the political activities of the Irish-Catholic bishops during a volatile period in the 1840s. In the mid-1840s, the bishops and their confreres in Rome had to respond to a triad of Irish reform measures framed by the government of Sir Robert Peel: the charitable bequests act, the augmentation of the grant to the seminary at Maynooth, and the attempt to found nondenominational

university colleges. Donal A. Kerr discusses each of these issues with great narrative clarity and with an amazing wealth of detail. There is no doubt that this work will be mined by historians for decades to come.

Some readers may object to the author's refusal to become involved, except in rare instances, in anything that borders on historical analysis. That objection, however, misses two points. The first is that Kerr intentionally has chosen a turn-of-the-century narrative style, and that choice must be respected. Second, on the few occasions when he does engage in analysis, he is embarrassingly inept. One example will suffice. Kerr gives a summary analysis of the history of education in nineteenth-century Ireland as follows: "Education, not emancipation, may well have been the major achievement of the Catholic church in Ireland during the nineteenth century" (p. 291). To see the church—not the Irish people, not the government—as responsible primarily for educational developments is a truly spiritual achievement, in this case one of mind over reality. This, though, may not be Kerr's fault, as he may have been misled by those who provided him with research notes: he thanks one Ambrose Macauley for furnishing him with "notes and transcripts concerning the national education controversy" (p. viii).

The volume is extremely well produced and has a good index.

DONALD H. AKENSON
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JOHN BOWMAN. *De Valera and the Ulster Question, 1917-1973*. Oxford: Clarendon Press of Oxford University Press. 1982. Pp. xiv, 369. \$29.95.

This is an extraordinarily sensible book. John Bowman has managed to distance himself from the received pieties of Irish political culture by clearly identifying six central myths within that culture and critically evaluating the evolving relationship of his subject to that myth structure.

Two of the myths, which we might call metaphysical, are that the Irish people are one nation rather than two and that a united independent Ireland is inevitable. In these myths, Bowman demonstrates, de Valera remained virtually a true believer throughout his long career. Indeed, the latter myth became his refuge, in a sense, when all efforts to end partition had come to naught. The other four myths are assertions about the practical politics of partition: that Britain deliberately partitioned Ireland for its own reasons; that if Britain were to abandon the Ulster Unionists they would be obliged to reach an accommodation with the south; that, indeed, Britain had the power to coerce the Unionists into a united Ireland; and that such coercion would lead

to a prosperous and stable country. Although de Valera never really succeeded—or indeed even tried—to understand northern Protestant opinion, the mere process of negotiating with nearly everyone who was anyone in British politics between the wars forced him to adopt a somewhat more realistic attitude, unlike many of his followers, with respect to each of these myths.

Bowman builds his case for this analysis on a painstaking reconstruction of de Valera's attitudes, pronouncements, and negotiations over the Ulster question from his emergence as a political leader after the Easter Rising to his effective retirement from party politics in 1959. His sources include a remarkable range of archival material from numerous repositories in Ireland, Britain, and North America. Ironically, he has not had access to de Valera's own papers, which are closed to 1985. De Valera's habit of never committing more to paper than was absolutely necessary, however, probably means that Bowman has missed little by publishing now.

The general reader in this country may find Bowman's reconstruction of events a bit too painstaking at times. Innumerable British statesmen were obliged to have interviews with de Valera over the years, and nearly all of them got the treatment that prompted Lord Granard to write "I have met men of many countries and have been Governor of a Lunatic Asylum, but I have never met anybody like the President of the Executive Council of the Irish Free State before" (p. 111). There were long periods in which nothing really changed except the identity of the individuals who were subjected to this treatment, and these sections of the narrative would have profited from a more ruthless editor. I suspect, however, that in Ireland itself the very richness of detail will widen the book's readership, which is fortunate, for this is indeed a work that has much to say not only to professional historians but also to the educated public in Ireland.

DAVID W. MILLER
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BARBARA KETCHAM WHEATON. *Savoring the Past: The French Kitchen and Table from 1300 to 1789*. Philadelphia: University of Pennsylvania Press. 1983. Pp. xxi, 341. Cloth \$35.00, paper \$17.95.

"Every period of French history has a characteristic meal type," writes Barbara Ketcham Wheaton. "In the Middle Ages it was the feast; in the sixteenth century, the collation; in the seventeenth century, the fête; in the eighteenth, the intimate supper" (p. 1). Wheaton addresses her book to historians, cooks, and "the student of French civilization" in a culinary history that extends from the end of the

thirteenth century to the French Revolution. The chronological boundaries would suggest that there is an "Old-Regime 'Type'" like C. E. Labrousse's economic cycle or Fernand Braudel's *moyenne durée*, while the meal typology hints at Philippe Ariès's approach to cultural mutations. In fact, Wheaton's intent is much more modest. She is interested in the elaboration, refinement, and rationalization of French *haute cuisine* for the court aristocracy and for the nation's noble and non-noble elites. Wheaton's book is based primarily on cookbooks, memoirs, published correspondence and literary sources, and a command of the considerable secondary literature on food habits, not only in France but also in England, Germany, and Italy. She is at her best describing the culinary art as it developed into a specialized and coherent *métier*. Now this requires more than a thorough knowledge of recipes and the rich vocabulary of food ingredients and their imaginative and complex manipulation by that ever more prestigious figure, the *officier de cuisine* or head cook. Wheaton lays out *la table* in the Braudelian style but in greater technical detail.

Although few readers of this beautifully illustrated book will be immune to the sensual and artistic impact of La Varenne's "garnishes of cockcombs, artichokes, and truffles surrounding legs of mutton and roast chicken" (p. 117), I was even more taken by the organization of the kitchen in 1650, especially in the large household. Wheaton thoroughly and artfully describes the central space assigned the kitchen in *hôtel* or *château*, the proliferation of culinary equipment (including the rotating spits, the array of ovens, the brick *potager* and chafing dish, the voluminous caldrons, casks, and *caves* for storage), and, finally, the elaborate staff hierarchy from commanding *maître d'hôtel* to fearful scullery maid.

I am not convinced that culinary change can be neatly linked to the publication dates of the great cookbooks, but there is no doubt that a mutation took place between 1650 and 1750 that should not be too closely associated with the magnificent court festivals of Louis XIV. Wheaton also points to the considerable social mobility near the summit of seventeenth-century French society, reflected in the consolidation of the *noblesse de robe*. These recent *arrivistes* used the fête and banquet to gain social acceptability and bought country châteaux and kitchen gardens to provide both the appropriate setting and the more tangible and palatable *délices de campagne*. One would also like to know more about food and drink as part of gift exchange. As the author suggests, the journal of Gilles de Gouberville opens a window on provincial food habits that would whet the appetite of a cultural anthropologist.

Wheaton is more concerned with charting and celebrating the evolution of *haute cuisine* than with

explaining it, but she does not fail to draw telling relationships with other developments and even moral conflicts in French society, such as dietary health versus elegant pleasure and pastoral simplicity versus urban sophistication—oppositions that parallel the culinary debate between the “transmutationist” cooks and the “purists,” between *déguisements* and the enhancement of the natural produce. Many of these issues are still with us or at least with those of us who have enough food, utensils, and time to “play” with the ingredients.

ROBERT FORSTER
Johns Hopkins University

JEAN-MARIE APOSTOLIDÈS. *Le roi-machine: Spectacle et politique au temps de Louis XIV*. (Arguments.) Paris: Minuit. 1981. Pp. 164.

Le roi-machine is neither a study of Louis XIV's government nor a history of early modern pomp and spectacle. It is an essay about how political power was organized, how the state perceived itself, and how it created an image of itself and the king. Jean-Marie Apostolidès develops his theories by drawing on the methods and ideas of such disciplines and bodies of thought as sociology, literary history, and Marxism. Many disparate elements of society, government, art, and religion are brought together to argue that France changed from medieval to modern in the early years of Louis XIV's personal reign.

The richest members of the traditional orders (clergy, nobility, and third estate) were not a class, according to Apostolidès. They were the “nation,” an elite group separated from ordinary people by their special relationship with the king. In addition to organizing culture in the academies, the monarchy used all the arts—painting, sculpture, literature, music, dance, and even fireworks—to resolve a number of contradictions in French society and to establish the predominance of the king. The person of the king was identified with Roman gods and medieval heroes in the myth-history stage of turning the monarch into the all-powerful and all-knowing embodiment of the nation. The image was perfected in famous festivities like the Pleasures of the Enchanted Island held at Versailles in 1664. With the Ballet of Flora the king became a hero in his own right without mythical ties. Versailles was turned into a magnificent permanent stage setting for the monarchy as the courtiers became satellites, entirely dependent on the Sun King. But politics, economics, and other forces could no longer be controlled by one man. After the image of the monarch solidified in the 1670s, the king withdrew from the real world and became machine-like, a “king ma-

chine,” while the royal place was gradually filled by the administration.

This essay is not intended for novices. Although little new information is presented (most is drawn from published sources and secondary works), a substantial knowledge of seventeenth-century French history and civilization is presupposed. The prose is opaque; important points are often hidden in the middle of incredibly long paragraphs. The vocabulary is drawn from so many different disciplines that meaning is often unclear. Much of what Apostolidès says seems very nebulous. Although his generalizations are usually plausible, they often seem to be based on flimsy evidence. It is never clear who or what (the king? intellectuals? the course of history?) was directing the image-making process. Relevant studies by non-French scholars like Robert Isherwood and Ralph Giesey are not mentioned.

Nevertheless, this “sociological analysis of the arts” should stimulate debate. No summary can do justice to the author's carefully constructed theory or do more than hint at his many subsidiary arguments. Even if one disagrees with Apostolidès's interpretations, they should not be ignored.

WILLIAM ROOSEN
Northern Arizona University

CHARLES JACQUES BEYER. *Nature et valeur dans la philosophie de Montesquieu: Analyse méthodique de la notion de rapport dans L'Esprit des Lois*. (Bibliothèque Française et Romane, series C, Études Littéraires, number 78.) Paris: Klincksieck. 1982. Pp. 385. 112 fr.

This work offers what Charles Jacques Beyer terms a *lecture naïve* of Montesquieu's classic *De l'esprit des lois*. He guides the reader chapter-by-chapter and section-by-section through Montesquieu's complex and unwieldy work, focusing almost exclusively on the idea of *rapport*. Beyer intentionally ignores almost all of the common debates about Montesquieu to see with greater clarity how Montesquieu himself perceived connections between the world of law and those of government, climate, religion, ethics, and the like.

Much of the commentary will be familiar to anyone who has read the original work with care. There are few surprises, but certain insights will be helpful, even to the expert. Beyer is admirably attentive to the nuances of the various ways of expressing *rapport*. He is also sophisticated in presenting Montesquieu's views on causal connections, emphasizing the “reversible causalities” (p. 302) that permeate so many of the philosophe's analyses.

All in all, however, the work is much too long and labored for what it yields. A considerably shorter essay that explored in detail the concept of *rapport*

and illustrated it with a few choice examples would have adequately served most readers. The continuous summaries that Beyer provides are almost as long as the original itself. Moreover, although a *lecture naïve* is a useful prologue to raising and exploring interesting historical and philosophical issues, it is still the issues themselves and the questions they provoke that are most stimulating and challenging. For instance, what is the nature of the social and cultural milieu that allowed and applauded the admixtures of fact and value, deduction and induction that later eras would try so hard to untangle? Or, how does this analysis of *rapport* lead to a broader reconsideration of Montesquieu's ideology and world-view, one beyond the traditional debates about his place in the conflicts of monarchy and aristocracy? Or, what genres penetrate the work, and to what extent do the various logics of connectedness mirror Montesquieu's kaleidoscopic comingling of previous genres of political writings (advice to princes, deductive political treatises, occasional pamphlets, histories of law, and so on)? The virtues of Beyer's book must be that it reminds us to read the classic with great care and provokes us to explore these issues anew.

Harry C. Payne

COLGATE UNIVERSITY

KENNETH MARGERISON. *P.-L. Roederer: Political Thought and Practice during the French Revolution*. (Transactions of the American Philosophical Society, number 73, part 1.) Philadelphia: The Society. 1983. Pp. 166. \$12.00.

Although specialists in the French Revolution know Pierre-Louis Roederer (1754–1835) in a variety of contexts, there exists no comprehensive biography of this important public official who rendered valuable service in the Old Regime, the Revolution, and the Napoleonic era. The failure to link his activities together has made him appear a superficial character whose motivations were mere responses to particular circumstances.

Kenneth Margerison's systematic analysis of Roederer's activities through 1799 provides a coherent interpretation of that part of the revolutionary's life. The author argues that the critical development leading Roederer to favor the Revolution was his fight in the 1780s against Old Regime monopolies that hindered businesses in Metz, where he served in the parlement. Such protests aligned Roederer with the emerging reform movement, where he participated with the Committee of Thirty and then was elected to the Estates-General. There he utilized his prior expertise to serve on the tax committee and was instrumental in ending the tobacco monopoly. His experience in this assembly made Roederer

a fervent supporter of the legislative process in general and committed him in particular to the Constitution of 1791 produced by this body. As such, he sided in 1791 with the Jacobins who did not want to water down the Constitution to please Louis XVI, and later, during the insurrections in June and August 1792, he unsuccessfully defended the constitutional monarch against the Paris crowd.

The ability of the crowd, despite Roederer's resistance, to overturn the government instituted by the Constitution of 1791 and to set up a republic encouraged him to shift his opinions. Forced into hiding, he developed a new set of ideals that reduced his earlier commitment to representative government. He asserted that studying nature would indicate the desirable goals of human endeavor and that the role of government lay in providing an order that would foster such ends. Furthermore, posited Roederer, although government might be somewhat representative, it had to be sufficiently insulated from society to permit careful reflection on how to promote the social good. At Thermidor, with the radical revolutionaries out of power, Roederer returned to the fray to implement his ideals. He supported all efforts to squelch the crowd that he believed thwarted orderly action. He associated with the royalists, not because they supported monarchy but because he felt their elite social background made them natural advocates of social peace. Although he backed the Directory to a degree, its instability convinced him that it could not create the necessary harmony. Consequently, he compromised still more of his belief in representative government by aiding Napoleon's overthrow of the Directory and erection of a regime that would ensure order.

Margerison certainly provides an account that satisfies his own goal of rectifying Roederer's rather disparate image. Scholars may dispute portions of his interpretation and will regret that a lack of personal papers forbids the testimony of the subject as well as his family and friends concerning private motives. More importantly, historians may wish that Margerison's goals were broader. Although the author ably employs his sources to contribute to significant historiographical questions, he does not develop new initiatives. One would hope to see the study of Roederer's life used a little more boldly. Such reservations notwithstanding, Margerison provides a very solid monograph.

JACK R. CENSER

George Mason University

CLIVE H. CHURCH. *Revolution and Red Tape: The French Ministerial Bureaucracy, 1770–1850*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xi, 425. \$59.00.

In this well-organized, informative, judicious study of the central administration of France, Clive H. Church provides the first comprehensive history of this institution and its personnel during the late eighteenth and early nineteenth centuries. His dispassionate analysis corrects a number of misconceptions about the French bureaucracy. He emphasizes that bureaucratization is a complex, dynamic process in which continuity and change are always present, but to varying degrees. Although he uses the period from Louis XV to the Revolution of 1848 as a context, he concentrates on the Directory (1795–99).

A number of important, well-considered themes run through the book. Each successive regime quickly found it necessary to retain most of the previous administrative apparatus in order to exercise authority. Even concerns about the political reliability of the bureaucrats—greatly publicized by contemporaries and historians—played a relatively limited role in transforming the administration. As these conclusions suggest, the relationship between revolution and bureaucracy is a complex one, not fully explained by general theories. According to Church, the most important division in the administration was that between the highest level of appointed or elective *fonctionnaires* and their more professional subordinates, the *employés* who kept the system operating. Finally, no matter what the regime, the French public maintained an ambivalent “love-hate” attitude toward bureaucracy that is the basis for many of the myths about it.

Although the prerevolutionary monarchy took some steps toward the modernization of the administration, its social and institutional structures prohibited genuine bureaucratization. Furthermore, despite legislative reforms by the Constituent Assembly, the organization, personnel, and functions of the central administration remained fundamentally unaltered until after the overthrow of the monarchy. The foundations of the modern bureaucratic state were laid only in the Year II (1793–94) when, in response to domestic and foreign crises, the state vastly expanded its responsibilities and personnel while establishing more rational regulations and organization for the civil service.

The Directory was the starting point of this expanded study by Church and it remains his primary focus. Like other recent historians, he believes that the achievements of this regime are inadequately appreciated. In the bureaucratic development of France the Directory constituted an essential phase when the innovations of the Convention were regularized and consolidated, for example, in storing information, increasing specialization, defining hierarchies, and clarifying administrative objectives. Simultaneously, the author points out that obstacles to complete bureaucratization re-

mained: financial instability, continuing patronage, absence of standards for recruitment and promotion, and uncertain tenure. Nonetheless, professionalism became more firmly established and *esprit de corps* developed, at least among the lower- and middle-level state employees.

In his discussion of the administration during the Napoleonic era, Church destroys one of the best-established myths about this period: that it was the “golden age” of bureaucracy. Like his predecessors, Bonaparte essentially continued the machinery that he found in place. Although he increased centralized control of the administration and further removed it from public influence, Napoleon’s concern was efficient execution of decisions; his attitude toward the administration and his treatment of it were highly personal. Whatever progress bureaucratization made during this period was due to developments within the administration rather than to direction from the government. Indeed, Napoleon’s appointment of notables to high administrative office often was contrary to bureaucratic principles and exacerbated the differences between these officials and their more professional subordinates.

The author’s brief discussion of the period 1814–48 demonstrates the persistence of patterns of bureaucratic development established over the previous quarter of a century. Typically, the Restoration made no significant contribution to the established system, but the July Monarchy fostered further professionalization and specialization.

As Church notes, little work has been done on the administrative history of France, and much remains to be done even after this solid study. One likely reason for this is that the subject has generally been viewed as boring. Unfortunately, Church’s treatment does little to dispel this impression; although convincing, his analysis is rather dull going, except for a few sections that provide individual examples of trends (for example, pp. 204–11 and 245–51). Regardless of whether this is the fault of the topic or the author, *Revolution and Red Tape* is an impressive book, informed by a theoretical understanding of bureaucracy, based on extensive research, and exhibiting balanced interpretations; it clarifies considerably a comparatively unknown and unappreciated aspect of French history.

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WILLIAM E. ECHARD. *Napoleon III and the Concert of Europe*. Baton Rouge: Louisiana State University Press. 1983. Pp. xiv, 327. \$32.50.

Napoleon III has perplexed historians almost as much as he baffled contemporaries. Was he vision-

ary or fraudulent, inept or prescient? Since he left almost nothing in writing and talked out of all possible sides of his mouth, documentation for his views has been hard to come by. As a result, no satisfactorily coherent account of his foreign policy as a whole has appeared up to now. But Lynn M. Case and his students have been attacking the problem for several decades. Longitudinal studies such as Nancy N. Barker's *Distaff Diplomacy*, describing the empress's input throughout the reign, have differentiated the policies of associates. Independently I myself produced a latitudinal study, *Napoleon III and the German Crisis, 1865-66*, in which I tried to isolate Napoleon's policy in a single episode. Now William E. Echard, by chronicling Napoleon's pet congress idea, has produced the most comprehensive, internally consistent account to come out of these twenty-five years of effort.

Echard contends that Napoleon's overriding goal was to satisfy the principle of nationalities and destroy the 1815 settlement by redrawing the map of Europe through general agreement in a European congress at the expense, where internal trading would not suffice, of the Ottoman empire. His method is to focus on successive plans for conferences or congresses, and since such proposals left a paper trail, which does lead plausibly to the emperor, he escapes from the paucity of sources. But there is a price to pay. Echard's Napoleon is too bureaucratic, too disinterested, and too much of a monomaniac. Echard misses the joyous mischief of the geography lessons given to Richard Metternich and the willingness to connive with institutional interests, such as the designs of the French silk industry on Syria, and he underrates other familiar Napoleonic techniques, ranging from the inspired pamphlet to the bilateral deal and limited war. Sometimes he reads too much between the lines in an attempt to show that every conference proposal was serious and every conference, in Napoleon's imagining, a general congress in embryo. Furthermore, there are limitations to Echard's knowledge of Europe beyond the Rhine and the Po: he seems no more aware than was Napoleon that the splendidly bold plans for restructuring the Balkans in the spring of 1866 had not a prayer of success while the Porte was under pressure from the Young Ottomans, who nearly carried out a successful coup in 1867.

Finally, although one cannot fault Echard for not having read every source on the period, there are some astonishing omissions from his bibliography, such as Nancy Barker's *Distaff Diplomacy*, already mentioned, or Luc Monnier's *Etudes sur les origines de la guerre de Crimée*, the only modern treatment to address French policy exclusively. A fuller use of Winfried Baumgart's *Peace of Paris 1856* would have provided additional ammunition for his thesis relative to the first years of the reign.

These criticisms are not meant to detract seriously from Echard's achievement. It is impressive.

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BARNETT SINGER. *Village Notables in Nineteenth-Century France: Priests, Mayors, Schoolmasters*. (SUNY Series in European Social History.) Albany: State University of New York Press. 1983. Pp. viii, 199. Cloth \$33.50, paper \$10.95.

This book deals with an important subject: village notables during a century when the French countryside underwent profound transformation. In six analytical chapters, priests, mayors, and schoolmasters are described in terms of social background, intragroup ambitions, and intergroup conflicts. The study cannot be faulted for inspiration or zeal of investigation. Barnett Singer worked hard in archival, particularly provincial archival, sources. He knows the French language, literature, and culture well and is sensitive to the subtle differences that characterize interpersonal and intersocial relationships. He has an eye for the amusing episode or escapade that turns up so frequently even in bureaucratic reports on local squabbles.

The study, however, lacks the analytical framework necessary for this energy and knowledge of detail to fructify. We are not talking about the author's decision to avoid numbers. Of course it would have helped to have some systematic statistical evaluation of priests, mayors, and schoolmasters, especially one that might have shown the evolutionary trends in group composition from regime to regime through this turbulent century. Statistics are missing, but the decision to turn to quotation instead of quantification can be respected.

Yet it is precisely in the qualitative analysis that problems arise. Most good works on social history discuss their subject within some sophisticated scheme of social dynamics. It is not particularly enlightening in itself to know, for example, that priests were poor, alienated, or subject to hierarchical discipline, or that priests and schoolmasters or priests and mayors were village rivals. These facts must be considered within a general context of French social change and political transformation. The appeal of the Tocquevillian, Marxian, or Weberian analysis is explained by this combination of the specific with the general. If, however, an author chooses to ignore sociopolitical analysis, is it possible for him or her to explore the psychological aspects of the notables' existence? The great works of nineteenth-century French literature do just that.

Barnett's book does not succeed fully because it eschews both the psychological and the sociological

task. There are references to the novels of Stendhal, Balzac, and Flaubert. There are also numerous quotations from primary sources that offer promising hints about the interior life of the notables. But references to characters in novels or short quotations from archival sources cannot be used as a substitute for sustained psychological descriptions. One can, to cite one example, get a much greater insight into the psychological and moral state of the French clergy in Anatole France's *Histoire contemporaine* than in these pages. Nor has Barnett entered into a prolonged discussion in the body of the book of the village notables as factors in the general evolution of French politics and society. Is it not necessary or even desirable for him to have imposed some general interpretative scheme on local history? It would even have been useful to know that the general schemes that have been so imposed are wrong. But the larger issues have to be consciously incorporated into the analysis of the village notables for this study to shed light on the significance of rural change in French history. Otherwise the discussion of the village notables tends, as most of their lives actually were, to become *petite histoire*.

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PHILIPPE LEVILLAIN, *Albert de Mun: Catholicisme français et catholicisme romain du Syllabus au Ralliement*. (Bibliothèque des Écoles Françaises d'Athènes et de Rome, first series, number 247.) Rome: École Française de Rome. 1983. Pp. 1062.

Fifteen years ago, Count Albert de Mun was best known to historians as a quixotic leader of the French right during its years of disaster between 1871 and 1914, a man whose name appeared in nearly every study of the Third Republic but about whom few details were available, despite the appearance of eleven largely hagiographic biographies during the fifty years after de Mun's death in 1914. Since 1970, three monographs have rescued de Mun from obscurity and demonstrated that his career summed up many of the contradictions and conflicts of his period: sequentially legitimist, Boulangist, and Rallié; anti-Dreyfusard; spokesman for some of the most advanced social reforms proposed in France before World War I; arch-defender of the Catholic church; and prophet of the nationalist revival between 1905 and 1914. Charles Molette contributed the first critical biography of de Mun in 1970; I added a second in 1978. The latest contribution is Philippe Levillain's *thèse d'état* (directed by Jean-Marie Mayeur), which has been published in a handsome edition from the École Française de Rome.

Until quite recently, it was believed that Albert de Mun destroyed all of his personal papers immediately before his death. Previous efforts to reconstruct de Mun's life have relied on his correspondence with colleagues and on his extensive published writings. Levillain (of the Université de Lille III) discovered that the papers in fact survived, held secretly by de Mun's family, and had the extraordinary good fortune to be granted access to the approximately thirty-six cartons of documents: wills, letters, minutes, memoranda, and private diaries.

From this material, Levillain has corrected the errors of all previous studies and added considerable depth and nuance to the portrait of de Mun as an aristocratic cavalryman marked for life by his Jesuit teachers, who devoted himself to noble causes. The suspicion that de Mun had a streak of egoism and arrogance is abundantly confirmed. New information about his high-handed direction of the Oeuvre des Cercles, the social Catholic organization he founded, reveals how de Mun imperialed (and thereby stultified) all other Catholic efforts at social reconciliation. De Mun's dramatic entrance into politics in 1876 gave the monarchist right a new champion but also, as Levillain demonstrates through new sources, aggravated de Mun's unjustified self-assertion.

The survival of documents relating to successions, dowries, and expenditures permits Levillain to make the first analysis of de Mun's economic position and the opportunity to examine him as a fin-de-siècle *rentier*. De Mun was concerned perpetually about the *placement sûr* of his investments and worried about increasing expenditures—but he was determined to maintain his aristocratic image through the education of his three sons at expensive Jesuit academies, a large dowry for his daughter, and the proper accouterments of clothing and furnishings for himself and his wife.

These additions and corrections are interesting and valuable, but they do not alter significantly the basic conception of de Mun established since 1970. There is nothing startling in Levillain's portrayal of a young aristocrat devoted to his vision of a utopia in which the abuses of labor, the antagonisms of social groups, and the erosion of religion (all of which he naively attributed to the growth of liberalism) would be ended through the elaboration of a corporatist society under the tutelage of the Catholic church. De Mun identified the Third Republic with liberalism and was led to adopt monarchy as the political form for his utopia. And therein lay the problem. The Bourbon *ancien régime* already suffered from the problems he deplored; the Orléanists and Bonapartists had adopted much of the liberalism he despised. De Mun could find the antique sentiments that matched his own idealism only in the hopeless

cause of the Comte de Chambord, and after Chambord's death in 1883 even that fantasy dissolved. As Levillain perceptively notes, de Mun was a monarchist because he was a Catholic, not because he believed in monarchism.

Levillain ends his biography in 1892, when de Mun rallied to the Third Republic at the request of Pope Leo XIII. The result is to deform the analysis of de Mun's career. In Levillain's view, de Mun, by making peace with his avowed enemy, forswore his utopia and admitted defeat in his role as the so-called Knight of the *Syllabus*. Another interpretation would be that the Ralliement opened an entirely new, and more realistic, course for a man of undeniable oratorical and organizational talents. If de Mun's victories after 1892 were limited in comparison to his ideal of a corporatist monarchy, they represented more success than he had known before the Ralliement.

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PETER SCHÖTTLER. *Die Entstehung der "Bourses du Travail": Sozialpolitik und französischer Syndikalismus am Ende des 19. Jahrhunderts*. (Campus Forschung, number 255.) New York: Campus. 1982. Pp. 300. DM 49.

Most students of the French trade union movement in the years before the First World War agree that one important force behind the rise of revolutionary syndicalism was the *Bourse du travail* movement. The *Bourses* were the municipally subsidized labor exchanges run by local trade unions. Despite the significance attributed to them, the *Bourses* remain poorly studied. As a result of the internal divisions and political tribulations of the French trade union movement in the twentieth century, the sources on the *Bourses*, where they exist, have been scattered over local archives or in private holdings. Perhaps most importantly, historians have lacked a conceptual framework for understanding the militancy of the *Bourses* and the nature of their appeal. Relying on original sources and a thorough knowledge of the secondary literature, Peter Schöttler surveys the prewar *Bourse* movement and proposes an important new interpretation of their role in the labor movement.

Schöttler stresses the dual nature of the *Bourses*. On the one hand, they were regional centers where workers came together and launched struggles, and as such they represented the workers' interests. On the other hand, most *Bourses* were dependent for their existence on subsidies from city governments committed to class harmony. The municipalities attempted to use their financial support to tame the workers' movement. French law further tried to

restrict the union movement by requiring trade unions to abstain from politics. Such restrictions were difficult to enforce against individual trade unions, but the threat to close the *Bourse* was often effective against radical workers who valued the services available there. According to Schöttler, the *Bourse* leaders capitulated under the threats of closure and the pressure of financial restriction. This capitulation was concealed behind anarchist rhetoric asserting that trade union organization should be the main goal of revolutionaries and denigrating union participation in politics. *Bourse* leaders accepted a false dichotomy between economic and social considerations and political goals to avoid the political involvement that might threaten their organization's existence.

Schöttler himself is not free from the dichotomizing of political and social concerns for which he criticizes the syndicalists. He confronts facts with theory where *Bourse* institutions and political issues are concerned but deduces from theory alone the degree of social organization and the potential for consciousness among French workers. Although he demonstrates that much in the so-called revolutionary *Bourse* movement might properly be labeled as reformist, he postulates that beyond the rhetoric of *Bourse* leaders, among the rank-and-file, revolutionary potential existed. Little evidence is presented to support this assumption. How well organized were local workers and what kinds of organizations did they form? What kinds of demands did embattled workers raise? Furthermore, the wisdom of *Bourse* leaders in compromising with the local authorities can be condemned only if it is true that such compromises headed off movement in a revolutionary direction. But would local trade unions have been strong enough to act militantly if they had been denied the aid supplied by the *Bourses*? Schöttler's summary description of how *Bourses* typically functioned is the best in existence but hardly conclusive on these questions. Paradoxically, Schöttler's attempt to reassert the primacy of the political only reveals the key importance of the social historical assumptions on which such an assertion must depend. This provocative study should provide incentive for scholars to examine local *Bourses* and the working-class organizations surrounding them.

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D. STEVENSON. *French War Aims against Germany, 1914-1919*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xiv, 283. \$49.00.

In 1961 Fritz Fischer published *Griff nach der Weltmacht*, initiating a decade-long debate over German

war aims in the First World War. Except for an article by Pierre Renouvin in 1966, no comparable study on French war aims has appeared. Polished and copiously researched, D. Stevenson's new monograph on French war aims toward Germany helps to rectify the situation.

Germany, Britain, and even the United States developed multiple, far-reaching war aims. France by contrast had only one war aim: the avoidance of defeat, then the pursuit of victory over Germany. Defeats—at Sedan in 1870 and in August 1914—set an immediate agenda for Paris: the return of Alsace-Lorraine, the evacuation of Belgium, and no separate peace. As the war continued, reparations and economic issues also emerged, but at first the only concern was victory: it constituted *the* French aim.

Not until late 1916 did the French move to draft a formal set of peace aims. Before those efforts could influence inter-Allied diplomacy, however, the disasters of 1917 began: Nivelle's mistaken offensive, the Russian revolutions, Briand's ouster, and American intervention, the latter simultaneously both a disaster and a blessing for France. With Russia's subsequent disappearance, the chief cornerstone of French diplomacy since 1892 vanished. The fate of Poland and the East European settlement gained new urgency, a fact that the author examines with freshness but disappointing brevity.

With Clemenceau's arrival in power in November 1917 the French decision-making process became more autocratic. At first he too had to worry about the mere winning of the war. The spring of 1918 looked like the summer of 1914, only more so—but with the Americans on the way. By mid-summer 1918 the situation differed entirely. Victory now seemed possible, the formation of peace aims imperative. Often shunning the Quai d'Orsay experts, the premier used his trusted lieutenants, especially André Tardieu, to prepare for the negotiations. In the process, Clemenceau virtually cast aside the traditional claimants from the economic sector. The French industrialists were background, not foreground, in Clemenceau's diplomacy. Raymond Poincaré also faded, as did Ferdinand Foch once the war ended and the peace conference began. For the French, Clemenceau was the policy process and for him security against Germany dominated every concern.

Stevenson is exceptional in describing Clemenceau's confrontations with Woodrow Wilson and David Lloyd George. He shows how the "Tiger" managed to get more of the French than Anglo-Saxon conception about Germany into the final treaty. Of course, Clemenceau preferred even more: a separate Rhenish state, the Saar, and extensive economic servitude for Germany. But he won occupation of the Rhineland, neutralization of its

west bank, the right to move into the Ruhr if reparations were not paid, and, as it turned out, the chimera of an Anglo-American guarantee treaty. In his desire for security, Clemenceau advocated an extensive Poland and no *Anschluss* between Germany and Austria. Deterrent alliances in the East and West, absolute control of some territorial security, and reparations shackles on the Germany economy were the guarantees of France's future security. "French policy therefore," Stevenson writes, "followed the usual tendency for a defensive inspiration to assume an offensive expression" (p. 213). The French in victory were no less territorial- or economic-minded than Chancellor Bethmann Hollweg. But the conditions of their victory limited their ability to translate their most extreme demands into reality.

The author has carefully exploited private paper collections and official files on the peace conference; no collection has escaped his notice. Occasionally, the study is more narrative and descriptive than analytical and conceptual, but the style is clear and the presentation convincing. It is a study of fundamental importance. Stevenson has opened the door wide on the operation of France's wartime diplomacy; works that follow will be dependent on his exploration. Few will be able to improve on his grasp of the essentially tragic nature, even in victory, of France's geopolitical situation. Clemenceau expressed it well when he muttered on November 11, 1918: "It will all be useless" (p. 214).

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J. A. FERNÁNDEZ-SANTAMARÍA. *Reason of State and Statecraft in Spanish Political Thought, 1595–1640*. Lanham, Md.: University Press of America. 1983. Pp. xxii, 353. Cloth \$25.00, paper \$14.00.

The subject of this monograph is an important one. Although the conflict between political realities and ethical standards is not as new as J. A. Fernández-Santamaría implies, the perception of this conflict by seventeenth-century Spaniards as well as their attempt to formulate a Christian reason of state is worth attention and deserves to be better known among historians of early modern Europe.

The author has carefully read a large body of literature and provides an elaborate analysis of themes and approaches. Various ways of confronting the problem posed and symbolized by Machiavelli form the architecture of the book. Some topics are handled quite well (for example, the excellent discussion of political deceit), but all too often the author's many qualifications and clarifications lead to a denseness that is difficult to penetrate.

Fernández-Santamaría's approach is purely doc-

trinal. The circumstances that provoked the attempt to formulate a Christian reason of state are mentioned only parenthetically in vague references to the challenge posed by absolutism (p. xii) or to a "feeling of crisis" (p. 318). The particular reasons why Spaniards might be especially concerned with questions of religious toleration are barely mentioned (p. 57). The author seems to regard the historical context that provoked the problem of reason of state and within which this body of thought was elaborated as either general knowledge or unimportant.

Doctrinal context is ignored as well. The author approaches his discussion of medical metaphors of the state in an interesting way, examining medical treatises. But he fails to explore either the origins of the metaphor in the medieval formulation of the body politic or Machiavelli's use of the metaphor in the *Discorsi*. The author notes similarities between writers but fails to analyze the possibility of influence. The perception of a conflict between principle and expediency was not new to the seventeenth century, but how the Spanish perception may have been influenced by other attempts to deal with the problem is not discussed. Once again, it appears that this context is regarded as general knowledge or as unimportant.

Most curiously, the author neglects even the scholarly context. As the extensive bibliography indicates, much attention has been given to the individuals and themes here studied. Yet, in both text and notes, the author avoids mentioning other studies and whether he is in agreement or disagreement with the views expressed in them. He presents biographical information on individuals without any reference to sources, although there is a reference to an anonymous "modern commentator" (p. 235, n. 66).

The book does have much to recommend it. The author's thesis that the early seventeenth century saw the culmination of a concern with the propriety reason of state and the beginnings of a concern with the nature of that reason of state is provocative, if not completely persuasive. Yet probably only a limited group of specialists will have both the stamina to penetrate the work's denseness and the knowledge to supply the missing contexts. Others would be better advised to consult the author's recent article, "Reason of State and Statecraft in Spain, 1595–1640" (*JHI*, 41 [1980]), which covers essentially the same subject and does so clearly and succinctly.

MICHAEL D. GORDON
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VICENTE PÉREZ MOREDA. *Las crisis de mortalidad en la España interior, siglos XVI–XIX*. Foreword by JORDI

NADAL. (Historia.) Madrid: Siglo Veintiuno de España. 1980. Pp. 526. 1,250 ptas.

Despite its title, this book is a significant summary of demographic trends in the interior of Spain during the Old Regime. By focusing on the question of mortality, however, Vicente Pérez Moreda clarifies a crucial issue in the social history of early modern Spain.

Until the latter part of the nineteenth century, the Spanish interior was a "safe harbor" for plague and epidemic disease. There were at least six major outbreaks of virulent plague between 1600 and 1800, and no generation of Spaniards during those two centuries escaped the mortal effects of disease. Consequently, scholars such as B. Bennassar have unhesitatingly ascribed the well-known underpopulation of the Spanish interior to the short- and long-term effects of pestilence. In treating this point, Pérez Moreda distinguishes between what he calls the "natural" demographic cycle and the periodic demographic "catastrophes" that characterized traditional demographic structures of the Old Regime. As opposed to other scholars, he does not find that epidemic catastrophes signal the termination of a demographic cycle. According to the author, the onset of plague was not a "natural" response to the Malthusian crisis. In fact, throughout the entire period 1600–1850, the Spanish interior registered a mortality rate of 37 to 43 per 1,000, which was considerably lower than the birth rate during the same two and one-half centuries. Moreover, the mortality rate and the low life expectancy (under 30 years) was primarily a function of infant mortality, including infanticide, which the author admits being unable to quantify in specific terms.

Given the lack of demographic catastrophes as a primary factor in the evolution of the Castilian population, how then does one account for the lack of people in the interior of Spain? Pérez Moreda quotes extensively from contemporary sources regarding the depopulated villages. The answer, according to the author, lies in the constant emigration of people away from the rural zones of the interior toward both the cities and periphery of the peninsula and the "promised land" of the New World. The strength of Pérez Moreda's narrative begins to fade somewhat when he attempts to quantify this thesis. Nonetheless, the author provides a valuable service and gives us a work of strong scholarship by focusing on the question of mortality as a whole.

The book suffers from one defect common to many recent Spanish monographs, namely, the obsessive concern with bibliographical review. Demography is no longer a stranger to the social sciences, and most readers will find the more than one hundred pages devoted to secondary literature a

little tiresome. The author's research is a clear verification of his scholarly credentials, and there is no reason to overload the text with references to every prior publisher in the field.

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C. R. BOXER. *Joao de Barros: Portuguese Humanist and Historian of Asia*. (Xavier Centre of Historical Research, Studies Series, number 1.) New Delhi: Concept Publishing. 1980. Pp. 159. Rs. 60.

It is regrettable that Joao de Barros, the dean of Portuguese chroniclers of Asia, has never been the subject of a comprehensive biography or exposition of his works in English. C. R. Boxer's present volume finally fills this gap.

Boxer's biographical sketch of Barros is based on Manuel Severim de Faria's account (1624) and is not intended to present anything new. We learn that Barros was educated at the Portuguese court by the small circle of humanists assembled there and that Barros was among the earliest native products of Portuguese humanism. From youth Barros enjoyed the patronage of Manuel I (1495–1521) and of his successor John III (1521–57), who promoted Barros in a noteworthy bureaucratic career.

Boxer devotes some space to Barros's many non-historical works. The first of these, the chivalric novel *Clarimundo* (1522), celebrates Portuguese overseas expansion and conquest and probably influenced Luis de Camões in writing his epic *Lusiades* a generation later. In the allegorical *Ropica Pniefma* (1532) Barros emulated Erasmus models, combining anticlericalism, social criticism, and advocacy of Christian pacifism. Although hampered by the obscurity of its own arguments, *Ropica Pniefma* established Barros as an important disciple of the Rotterdam humanist. The highly original grammatical and didactic works (1539–43) place Barros in the vanguard of educational reform in Portugal and the promotion of a greater appreciation of vernacular Portuguese. Unfortunately, these latter works made little impact on his or succeeding generations.

Boxer reserves rather more space for the *Decades of Asia*, on which is based Barros's fame among modern readers. Conceived as part of a world history, four of the *Decades* were completed and published in 1552, 1553, 1618, and 1628. Although Barros never traveled to Asia and probably ventured to Africa on one occasion only, the *Decades* demonstrates Barros's remarkable appreciation of the geography and cultures that the Portuguese encountered in Africa and Asia. Barros overcame language and cultural barriers to become a notable Orientalist and was certainly the first European to employ native Chinese and Persian sources, which

he had translated for him. Despite his admiration for the *Decades*, Boxer does not neglect criticism of Barros the historian. He cites, for example, Barros's partisanship, his sometimes trivial reporting, and his failure to assess sources critically. On all of these counts, however, Boxer finds that Barros far surpassed his contemporaries.

Boxer succeeds in his avowed purpose—to make Barros and his works better known to English readers. But he has set himself a modest goal, perhaps too modest. By disclaiming the role of literary critic, Boxer must rely on analyses by Barros's major critics—Israel Salvator Révah, António José Saraiva, and José Sebastião da Silva especially—and it is to them that the reader must turn for more than an introduction to Barros's writings. Boxer does include a well-annotated and complete bibliography for the serious student.

Printed in New Delhi, the volume is up to neither usual editing nor production standards.

JAMES C. BOYAJIAN
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CARL A. HANSON. *Economy and Society in Baroque Portugal, 1668–1703*. Minneapolis: University of Minnesota Press. 1981. Pp. 354. \$22.50.

The history of Portugal in the last quarter of the seventeenth century has attracted little attention from historians. It is usually viewed as peaceful but stagnant era between the turbulent years of the war of independence (1640–68) and the comparative prosperity and expansion of the first half of the eighteenth century. Carl A. Hanson subjects the conventional interpretation to a reexamination and shows that the age of Dom Pedro II was a period of important socioeconomic change. The principal theme of this well-written book is the struggle between the commercial class and the privileged estates (clergy and nobility) for control over Portuguese economy and society in the seventeenth century.

The initial chapters are devoted to a class analysis of seventeenth-century Portuguese society, but the author creates unnecessary problems for himself and weakens his argument by using French terms such as the "Third Estate," the "nobility of the robe," and so forth to describe a very different Portuguese situation. This is followed by a perceptive discussion of the position of the New Christians (persons of Jewish ancestry) and the commercial class in general during this period. The core of the book is dedicated to a study of mercantilist thought and practice in Portugal, and the last chapters deal with the expanding national and imperial economy in the last decade of the seventeenth century.

The commercial depression of the 1670s led the

Portuguese government, influenced by French mercantilist thought, to attempt to develop domestic manufactures. Under the count of Ericeira—the Portuguese Colbert—a variety of programs were undertaken to stimulate internal development, but, despite some initial gains, these efforts were unsuccessful. The revitalization of the Portuguese economy was closely linked to the strengthening of the merchant class whose members were mostly New Christians. The author sees the renewed activities of the Inquisition against the New Christians largely in terms of class struggle, although he admits that greed and antisemitism were also present. The conflict between the merchants and the privileged classes had existed for centuries in Portugal, but the suppression of the commercial class during the reign of Pedro II marked an important victory for the established order. “Nearly a century was to pass before the mercantile class again presented a significant challenge to the stability of the Old Regime” (p. 70).

The failure of the merchants’ challenge to the Old Order ruined the mercantilist program, but coincidentally, in the 1690s, there was a new period of commercial expansion derived from the export of Brazilian gold, tobacco, hides, sugar, and Portuguese wine. Using the dependency theory, the author shows how the Luso-Atlantic world became increasingly dependent on England, a relationship that was confirmed in the Methuen Treaty of 1703. The Old Order triumphed and with it an economy based on the exchange of cash crops and mineral wealth in return for English manufacturers, foodstuffs, and other products.

The author of this book has done extensive archival research and has made skillful use of his sources. His work contributes much to our understanding of Iberian history in the early modern era.

RUTH PIKE
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L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* [The Kingdom of the Netherlands in the Second World War]. Volume 10b, *Het laatste jaar II* [The Last Year, pt. 2]. In two parts. The Hague: Martinus Nijhoff, for the Rijksinstituut voor Oorlogsdocumentatie, Amsterdam. 1980; 1982. Pp. vii, 763; vii, 765–1543.

Volume 10b of L. de Jong’s *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* covers the period from October 1944 to the end of the occupation in May 1945. During this time span the southern Netherlands below the Great Rivers were under Allied control, but the rest of the country remained under German occupation until the liberation of the

eastern and northern provinces early in April 1945. In Volume 10b de Jong deals primarily with events in the sections still occupied by the Germans.

The western part of the country contained the big cities and almost half of the Dutch population. Its inhabitants experienced a traumatic winter filled with terror and hunger as a result of the embargo the Germans clamped on transportation in an attempt to break the Dutch railroad strike. Because of this “transportation crisis,” the population of the western provinces suffered from serious famine for the first time since the Middle Ages. Twenty-two thousand persons died from malnutrition. De Jong characterizes this “winter of famine” (*hongerwinter*) as “one of the great calamities of Dutch history” (p. 275).

De Jong assigns direct responsibility for this catastrophe to *Reichskommissar* Seyss-Inquart, who tried to use the people of the western provinces as hostages to end the strike. The author describes most vividly the quality of life in the west during this ordeal. Each day an estimated 50,000 persons traveled by bike, boat, or on foot from the cities into the surrounding countryside or to the eastern agricultural provinces, in order to buy from peasants food that might help their families to survive. De Jong believes that Dutch peasants on the whole tried to be helpful and that the negative and derisive picture of peasant greed and selfishness in the underground press was grossly overdrawn.

During this time the Dutch resistance became more active than ever. Some degree of coordination had been achieved through the appointment of Prince Bernard as commander of the Forces of Interior (*Nederlandse Binnenlandse Strijdkrachten*) and through the inclusion of a variety of resistance groups within that framework. Furthermore, members of the resistance were represented in such coordinating bodies as the *Contact-Commissie* and in the *College van Vertrouwensmannen* (Council of Representatives), a group of individuals designated by the government-in-exile to serve as deputies of the government in the interim between the German collapse and the arrival of the Dutch government.

Unfortunately, this coordination remained limited since each resistance organization and each local group remained fiercely insistent on their independence. Moreover, the approach of liberation precipitated a divisive jockeying for positions of power during the transition period, and it produced increasing disagreements over postwar politics. De Jong describes in great detail these internal conflicts within the resistance and shows that even before liberation traditional political forces intent on re-establishing the prewar system of parliamentary government managed to checkmate successfully those elements on the left that hoped to produce a fundamental renewal of political life and structures

under the auspices of the resistance. Within this context, socialist and conservative politicians were determined to frustrate any attempt of the Communist party to take over the leadership of the working class through a socialist "Unity party" or a "unity trade union." De Jong has special words of praise for the work of the *College van Vertrouwensmannen*, which was virtually abandoned by queen and government on the eve of liberation and supplanted almost immediately by a military government.

In the concluding pages de Jong gives a masterly portrayal of the ambivalence of liberation. Despite the elation of the moment, the Dutch faced not only a largely ruined country but also, worst of all, in the month of May, uncertainties about hundreds of thousands of their countrymen deported to Germany and the East whose fates were unknown. Yet his description also makes it clear that the basic fabric of society had remained intact and that the Dutch emerged from the trauma of the occupation with a high degree of self-esteem and with less internal dissension than their neighbors to the south.

Even though volume 10b covers the end of the occupation and of the war in Europe, two more volumes are to be published. One of these will deal with Indonesia during the war. The final volume will attempt to place the Dutch experience into its broader historical context.

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OLE FELDBÆK. *Danmarks historie*. Volume 4, *Tiden 1730–1814* [History of Denmark. Volume 4, 1730–1814]. Copenhagen: Gyldendal. 1982. Pp. 383. 260 KR.

Ole Feldbæk has produced a very well-balanced and useful survey of Danish history during the 84-year period stretching from the accession of Christian VI in 1730 to the fateful year 1814 that witnessed Denmark's loss of Norway. Appearing as volume 4 in the major new *Danmarks historie* published by Gyldendal (volumes 1 and 2 appeared in 1977 and 1980, volume 3 has yet to appear), Feldbæk's work is not only a major interpretation of the period but also an indispensable reference work for scholars wishing to delve into this period of Danish history.

The volume opens with a characterization of Denmark between 1730 and 1814 and with detailed presentations of the available archival sources and of previous surveys of the period. A broad chapter on "Denmark circa 1730" follows, which sets the stage for the various lines of discussion Feldbæk intends to pursue in the remaining chapters. These subsequent chapters are thematic in nature, treating "State Power and the Political Elites, 1730–1814"; "The State and the Development of Society, 1730–

1814"; "Rural Society, 1730–1807"; "Town Society, 1730–1807"; "Beliefs, Knowledge, and Attitudes, 1730–1814"; "Foreign Relations, 1720–1807." A final chapter discusses "The Wars, 1807–1814."

At the end of each of the eight chapters there is a survey of the secondary literature dealing with that particular topic, and the volume would be well worth at least half its price if it only included these surveys and the 26-page presentation of sources, reference works, and published documents at its conclusion. These surveys of the literature serve the function of carefully annotated bibliographies, and their magnitude is reflected by the fact that they average five pages in length. Indeed, the bibliographical and archival surveys and discussions, together with the long bibliography that closes the volume, account for nearly one-third of the book.

Feldbæk's approach is a refreshing one. Although volume 4 of *Danmarks historie* will serve for several years to come as the primary reader for university courses on eighteenth-century Danish history, the author states from the outset that his is what he calls "an absolute form of presentation, which is to be understood thus, that the presentation expresses the author's interpretation and understanding" (p. 14). Thus Feldbæk emphasizes the social, economic, administrative, and cultural changes Danish society underwent in the period at hand while giving political and diplomatic events and developments an adequate but not overly detailed presentation.

Feldbæk, like his colleagues in volumes 1 and 2, has given the scholar and the student alike an exceptionally firm point of departure for gaining a command of the secondary literature and of the conceptual debates that have concerned historians and others vis-à-vis eighteenth-century Denmark. It is a masterful approach that has produced a useful tool; the Gyldendal approach could be used with advantage as a prototype for other national histories.

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SUSAN SEYMOUR. *Anglo-Danish Relations and Germany, 1933–1945*. (Odense University Studies in History and Social Sciences, number 78.) Odense, Denmark: Odense University Press. 1982. Pp. 295. 158.60 KR.

Susan Seymour's thesis, on which she received a PhD from the University of London, makes a substantial contribution toward illuminating the English attitude toward Denmark while Hitler held power. Relevant Foreign Office and other official British files were available to her, and she obtained access to Danish Foreign Ministry documents covering the time until April 1940. Moreover, she estab-

lished close contacts with leading Danish historians who have dealt with the occupation period and the years preceding it. The result is a scholarly work characterized by fine insights and sound judgments.

Britain's prewar interest in Denmark was confined almost exclusively to obtaining quality food, namely bacon, butter, and eggs. Seymour examines the Danish-English trade negotiations in great detail. She shows that the British did not consider the Danish food supplies very important, since supplies could also be obtained from the Dominions. Denmark depended to a certain extent on fodder from Great Britain, and the British concluded, mistakenly, that if Germany occupied Denmark and the fodder supplies were cut off, Danish agriculture would be able to supply only the domestic market and would not be of particular importance to Germany. Danish fodder production had in fact increased to such an extent that, when the occupation came, Denmark could redirect almost its entire agricultural export to Germany, and Germany benefitted considerably from Danish food during the war.

British documents relating to the Special Operations Executive (S.O.E.), the cloak-and-dagger organization that supported resistance movements in occupied countries, were not made available to Seymour. Consequently, she says little about military affairs. She has concentrated instead on the attitude of British officials toward the Danish cabinet, which held office until 1943; toward Danish exile groups; and toward the Freedom Council, the leadership organ of the Danish resistance movement. Her description of John Christmas Møller's role in England is especially good.

Although there is no doubt about Seymour's scholarly attainment in this thesis, the work is not well written. The author has a tendency to engage in long, clumsy sentences, the paragraphs sometimes lack coherence, and the book is redundant and marred by countless typographical errors. The manuscript thus would have benefitted from going through the hands of a competent editor. In spite of these minor drawbacks, Seymour's work will be read with interest, especially because so few substantial works in English deal with Denmark during the Second World War.

WILLIAM D. ANDERSEN
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BIRGITTA OLAI. *Storskiftet i Ekebyborna: Svensk jordbruk-sutveckling avspeglad i en östgötaskocken* [The First Phase of the Swedish Enclosure Movement—the *Storskifte*: Swedish Agricultural Development as Reflected in a Parish in Östergötland]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 130.) Uppsala: Historiska Institutionen vid

Uppsala Universitet; distributed by Almqvist and Wiksell International. 1983. Pp. 289. 130 KR.

This excellent PhD thesis by Birgitta Olai is a meticulously detailed study of the *storskifte* in one Swedish parish. The *storskifte*, which was the first of three steps in the Swedish enclosure movement, occurred during the second half of the eighteenth century. This reform reduced the number of separate plots in the arable "infields" and converted common "outfield" land into private plots. It was only the *lagaskifte* of the nineteenth century, however, that ended the two-field system of cultivation and broke up the medieval villages.

The extraordinary availability of written records, including maps, makes Sweden an ideal country for this kind of microstudy. Despite the favorable situation with regard to sources, Swedish microstudies cannot escape the principal drawback of all such work: the problem of generalizing local results to a larger context. In a bureaucratic country such as Sweden, it is probably quite safe to generalize about administrative and legal aspects of the enclosure movement. Other local findings, dealing with questions such as the economic motives for and results of the enclosures, the extent of plot consolidation, and the increase in land under cultivation, are more dangerous to generalize to a national level. Even when Olai's results cannot immediately be applied to the whole country, they are suggestive. Furthermore, they frequently tend to undermine the conventional wisdom (usually generalizations based on no detailed studies whatsoever).

Olai argues that the cause of the *storskifte* was not, as is usually argued, a bright idea in government circles. Rather, it resulted from a growth in population that required more grain production, together with an exhaustion of the supplies of unplanted land within the infields. Thus crops could only be increased, within the two-field system, by more efficient organization (that is, plot consolidation) or by enclosing, and planting, common outfield land. The author supports this argument with an ingenious examination of the changing extent of arable acreage.

This line of argument is also supported by the observation that the peasants were at least as likely as the members of the upper classes ("persons of rank"—*ståndspersoner*) to instigate enclosure. Once again this conclusion is contrary to the received wisdom. Olai also reports that, although the peasants were somewhat less likely than were persons of rank to appeal the preliminary results of an enclosure, they were more likely to win their cases in court. These facts seem to be in good accord with the argument, made by myself and others, that the Swedish peasantry of the eighteenth and nineteenth centuries was unusually well educated and sophisti-

cated and enjoyed a superior political and legal position, compared with peasants elsewhere in Europe.

It is difficult to find anything of importance to criticize in this well-researched and well-written book. An economist might be surprised that the author should even bother to argue that the true motive of the individual instigators of enclosure might have been crass economic self-interest rather than the more noble motives proclaimed in the enclosure petitions (p. 55). Similarly, to say that the consolidation of infield plots, on the one hand, and the division of the common outfield into private plots, on the other hand, represented "opposed principles" (p. 58) is not very illuminating. In fact, both steps were in accord with the underlying principle of creating less complex and more complete property rights in land.

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MARK U. EDWARDS, JR. *Luther's Last Battles: Politics and Polemics, 1531-46*. Ithaca, N.Y.: Cornell University Press, 1983. Pp. xii, 254. \$19.95.

In some respects the present volume is the continuation and completion of Mark U. Edwards, Jr.'s first book, *Luther and the False Brethren* (1975). Both works concentrate on Luther's often violent and scatological polemics, a subject customarily avoided by scholars sympathetic to the reformer. Edwards's intent here is not to discredit Luther but to understand the entire man. This means not only to view the vitriolic and the genial together but also to relate Luther's polemical treatises to the settings in which this sixteenth-century celebrity held forth.

In *Luther and the False Brethren* Edwards examined Luther's reaction to those within the evangelical movement who disagreed with him. In his new book he turns to the reformer's verbal attacks on individuals and groups outside the Protestant fold: Albertine Duke George, proponents of a general church council, the Turks, the Jews, Duke Heinrich of Braunschweig-Wolfenbüttel, the archbishop of Mainz, the bishop of Naumburg, and, repeatedly, the pope. The author finds that as Luther grew older he became harsher, more ready to vilify his adversaries. He sees the explanation as lying in the reformer's deepening apocalyptic convictions and his "Augustinian view of the dynamics of history." The Last Judgment was at hand, and Luther thought himself God's agent in bringing this to men's attention. As human history drew to its close, God threatened Germany with two rods, the Turks and the papists, for its "shameful reception of the Gospel." Luther viewed the Jews like the Sacramentarians, as subverters of the Gospel from within Germany and thus as servants of the devil.

Edwards gives convincing evidence of Luther's susceptibility to politics and politicians. He agrees with Eike Wolgast that Luther was increasingly required by Elector Johann Friedrich not to advise conscience but to relieve consciences. The prince involved Luther in his struggle against Heinrich of Braunschweig-Wolfenbüttel, requesting him to employ his talent for picturesque defamation against this duke. The Schmalkaldic League looked to Luther for arguments against the Council of Trent. When Johann Friedrich needed to justify his political aim to take over the diocese of Naumburg, he approached Luther and the Wittenberg theologians. The estates of the diocese elicited from the reformer a statement strikingly at odds with his early teaching on obedience to secular authority.

This study focuses on the neglected older man. It is a small though debatable point whether the Luther portrayed in this book was indeed an older man only after 1530. He had passed the midpoint of his life nearly three years before he wrote his Ninety-five Theses. He was middle-aged throughout the early Reformation. It might also be discussed whether Luther was, as the author maintains, abusive or mild only when he chose to be. Luther will probably continue to be seen as a man who could not always control his temper. Be that as it may, Edwards has celebrated Luther's quincennial by illuminating the reformer's thought and personality in a way that could never be achieved by studying the man's words alone. Future historians will identify Edwards's book as one of several that marked a turning point in Luther research. No one interested in the Reformation can afford to ignore it.

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HELMUT NEUHAUS. *Reichsständische Repräsentationsformen im 16. Jahrhundert: Reichstag—Reichskreistag—Reichsdeputationstag*. (Schriften zur Verfassungsgeschichte, number 33.) Berlin: Duncker und Humblot, 1982. Pp. 625. DM 240.

It is difficult to evaluate this study. After working my way through some six hundred pages of exceedingly dry prose, I am not at all certain as to just what I have learned. Helmut Neuhaus has no thesis as such. What he offers is a set of descriptions detailing each of the parliamentary experiments in sixteenth- and early seventeenth-century imperial politics. The list is surprisingly long; the descriptions disappointingly flat.

In both instances we get a detailed account of what could be called the mechanics of meeting: the list of delegates, the specifics of date, time, and place, the agenda for deliberation, the number of sessions, the number and kind of subcommittees, and the

rules of order governing the debates. But no effort is made to set these institutional details in the broader context of contemporary life. It is as if the assembly itself were the only reality. Its debates and failures are taken at their face value. Neuhaus never seems to realize that the behavior he so meticulously chronicles might itself derive from—and hence provide perspective on—the social, economic, and cultural pressures within German society.

These pressures need not in themselves have concerned Neuhaus. My point is not to criticize him for writing an institutional rather than a social history. It is rather to insist that, for an institutional history to succeed, it must be written in such a way as to make explicit the links between parliamentary behavior and the society out of which the participants come. Otherwise one is left with nothing more than a description of success or failure. Underlying questions of causality remain unanswered. And this is exactly what happens in Neuhaus's study.

Neuhaus charts the fate of two little-known sixteenth-century reform institutions, the *Reichskreistag* and the *Reichsdeputationstag*. The first of these was a national assembly composed of at least two delegates from each of the ten imperial Kreise. It developed independently of the Reichstag. At three moments—1535 (Worms), 1554 (Frankfurt), 1567 (Erfurt)—it looked as if it might even supercede the Reichstag as the governing organ of the empire. But in each instance the assembly lost its momentum and foundered on the opposition of the electoral princes. The *Reichsdeputationstag* was less innovative. It remained tied to the Reichstag and functioned rather like a standing committee charged with specific and limited responsibility.

We follow each of these meetings without ever understanding the underlying cause of their occasional successes and more consistent failures. It comes as no surprise that the electors opposed a national assembly in which each delegate exercised an equal vote. They recognized that such a body challenged the control that the collegial organization of the Reichstag assured them. It is decidedly less clear why the others went along with the electors. And since Neuhaus never considers the dynamics between the delegates and their individual constituencies or the particular needs of either Kaiser or Reich, the assemblies never come alive. The actors appear simply as puppets, manipulated by unseen hands according to a script to which we are not privy.

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KARSTEN HARRIES. *The Bavarian Rococo Church: Between Faith and Aestheticism*. New Haven: Yale University Press. 1983. Pp. xv, 282. \$37.50.

The greater part of Karsten Harries's sophisticated and imaginative book consists of a detailed aesthetic analysis of the Bavarian rococo church. The type has been widely discussed by art historians, who have not always agreed on the characteristics that qualify a particular building as belonging to it. As a working definition, the author accepts Bernhard Rupprecht's five criteria. (1) A central space is created, illuminated by indirect light. (2) The boundaries of this space remain indefinite. (3) Traditional architectural forms are changed or masked. (4) An ornamental stucco zone is placed between fresco and architecture. (5) A point of view near the entrance is of great importance for the design of the interior space. From this position the central space is seen in its entirety and appears as a pictorial whole. By applying and further refining these categories, the author seeks to discover the essence of the type, significant attributes of which he finds in the illusionistic quality of the interior space and in the fluid interplay between architecture and fresco, an equal partnership "mediated" by ornamental zones between the fresco and its adjoining arches and walls (p. 236). But he is even more concerned with interpreting the origin of the type and the reasons for its disintegration from the 1760s on.

Aesthetics alone cannot explain the rise and decline of a style. When exploring such issues, aesthetics must combine with the history of ideas and take into account political and social factors. According to Harries, the development of several generations of highly accomplished peasant artist-craftsmen, who gave the Bavarian rococo its pronounced popular character, depended on the piety and traditionalism of rural society in a country, still largely untouched by the Enlightenment, in which the church remained the decisive cultural force. The author's approach is genuinely interdisciplinary, but it is his highly technical aesthetic analysis that lends authority to his interpretations of the social and ideological conditions that determined the brief flowering of the Bavarian rococo church.

If Bavarian piety and backwardness were essential for the growth of the style, they also helped cause its downfall. As the Enlightenment penetrated the court and the bureaucracy, the desire to improve the economic conditions of the peasant and to free him from superstition and ignorance came into conflict with the exuberant theatricality and economic wastefulness of the country's religious life. The government reduced the number of church holidays and prohibited passion plays. In 1770 the elector issued a mandate removing design control of new churches from the local monastery or parish and placing it in the hands of a central board that would eliminate "all superfluous stucco-work and other often nonsensical and ridiculous ornaments," fostering instead "a noble simplicity appropriate to

the veneration of the sanctuary" (p. 196). Reformist bourgeois values were imposed on a largely unsympathetic peasantry; neoclassicism gradually took the place of rococo.

The author points out that modernist dislike of a highly decorated and extremely expensive style of church architecture, and of its social implications, was only one reason for the decline of the Bavarian rococo church. Another lay in the style itself. Its peculiar integration and blending of architecture, painting, and ornament could not long be maintained. Each of the three elements developed further, seeking primacy or autonomy, until the balance broke down and turned into a new synthesis. That neoclassicism did not reflect popular feelings with the immediacy and power of the Bavarian rococo seems certain. The author's further suggestions that the disintegration of this style marks the separation of faith and art and the emergence of a modern aesthetic—art for its own sake—seem more problematic, if unusually challenging. Scholarship rarely does more than hint at the richness and substance of historical reality, but some of the intellectual and artistic playfulness, of the gravity and universality of the Bavarian rococo church has become part of this splendid book.

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KNUT BORCHARDT. *Wachstum, Krisen, Handlungsspielräume der Wirtschaftspolitik: Studien zur Wirtschaftsgeschichte des 19. und 20. Jahrhunderts*. (Kritische Studien zur Geschichtswissenschaft, number 50.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 302. DM 78.

This volume is a collection of articles published by Knut Borchardt during the last twenty years. Borchardt, an economist and historian, wanted to place actual economic problems "in historical perspective" and to ask "how far is it possible to learn from history?" Three articles discuss economic problems from a historical perspective. In the first of these, entitled "European Economic History Can Be a Model for the Developing Countries," the author argues that it is more relevant to study the pre-industrial European states, which could imitate the English model, in order to devise models for developing countries than it is to study the English case (the only real Industrial Revolution). Due attention must be paid, however, to the difficulties generated by the time lag and by the special situations that exist in the Third World (explosion of population, problems of social adaptation, and the role of the state where private entrepreneurship is missing). The next article in this group compares the economic fluctuations of the period after World War II with those of pre-World War I and the period between the two wars. Through use of a series of indicators,

Borchardt shows that even the pre-World War I period suffered fluctuations that characterized the so-called cycles of growth of the post-World War II period. The third historically based article concerns the decisions taken by the German government during the world economic crisis, especially after the British government went off the gold standard. Within a scheme of possible options that seemed open to the German government, Borchardt stresses the necessity for a more detailed analysis of the comparative advantages of each option.

Most of the articles deal with themes that may help to determine how far changing experiences of the actual situations that occurred in recent decades and of the new scientific knowledge facilitate a better analysis of the past. The whole volume of articles is divided into three sections. The first section, having four articles, is dedicated to the conditions of economic growth during the nineteenth century. Concerning the famous question of how far shortage of capital retarded the process of industrialization, Borchardt finds that there was no shortage and that other factors must be considered: moderate chances for profit, greater risks compared with the more favorable possibilities of investment in other sectors, limited demand for investors, and a strong demand for liquidity (pp. 28–41). Regional differentiation of growth seen from the typical West-East gradient is, for lack of sufficient sources, investigated with supplementary material (physicians' approbations and records of visitors to secondary schools), with an epilogue discussing the thesis of J. G. Williamson (pp. 42–59). Growing interest for expenditures in the sector of education at present, not as an aspect of consumption but as a part of infrastructural investment, is the background for the statement that in nineteenth-century Germany the state mainly financed the primary schools (pp. 60–70).

The second group of articles deals with the various patterns of economic development in nineteenth- and twentieth-century Germany. Here Borchardt explains his views and presents three models for analyzing the main tendencies of long-term movement: trend, cycles, and irregular components (pp. 100–124). Another aspect discussed in this section is the economic development of the Federal Republic of Germany and the extent to which it was the result of disruption or a phenomenon of reconstruction (pp. 125–50). The two major inflations are primarily seen as political phenomena, which models of monetary theory help little to explain (pp. 151–61).

The main issue of the three articles of the last group (pp. 165–224) is indicated by their common title: "The Structural Weakness of the Weimar Economy and the Possibilities to Act during the Big Crisis." The abnormal economic development of the

so-called "middle years" is primarily explained in political terms: the state was weak and the tariff partners lacked responsibility or, put another way, the policy of distribution dominated to the disadvantage of a policy of production. Burdened also with the necessities of foreign policy, the Brüning government had little choice. As a whole, the book provides a stimulating analysis of the German economy from the nineteenth century to the *Wirtschaftswunder*, and is notable for its methodological perspectives. Here the discussion remains open; seen within the complexity of history, the periodization on the basis of economic facts has to be supplemented by other aspects. A lengthy section of notes (pp. 227–94) and supplemental materials enrich the basis of the discussion.

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ECKHART GRÜNEWALD. *Ernst Kantorowicz und Stefan George: Beiträge zur Biographie des Historikers bis zum Jahre 1938 und zu seinem Jugendwerk Kaiser Friedrich der Zweite*. (Frankfurter Historische Abhandlungen, number 25.) Wiesbaden: Franz Steiner. 1982. Pp. ix, 189. DM 48.

As a subject for biography Ernst Kantorowicz has great potential. Had he not written his famous study of Frederick II, first published in 1927, he would still be an intriguing example of the odyssey of the modern Western, and particularly modern German, sensibility from an exuberant vitalism through a despairing existentialism to, in the end, silence.

A Jew born in Poznań and thus an outsider in so many ways, Kantorowicz because of his experiences as both a front soldier and as a Free Corps fighter appears to have been attracted to the idea that he was among those who represented a "secret Germany," a spiritual aristocracy that would lead its country toward a vibrant future. In Heidelberg after the war Kantorowicz became involved in the George circle, one of the many groups that attempted to translate these notions into some sort of reality. He remained one of Stefan George's intimates until the "Master's" death in 1933 and clearly took inspiration, both for the subject of his historical work and for his approach, from the coterie surrounding this particular Führer, this artist-priest.

Kantorowicz did, it must be said, contribute unwittingly to the victory of a vulgar and virulent right-wing nationalism in Germany, and when he was then rejected in 1933 like so much flotsam even by many of his old friends, his entire intellectual endeavor was called into question. That Himmler and Göring read the Frederick biography and that Hitler apparently read it twice casts a blanket of irony and perhaps even absurdity over Kantorowicz's effort. In view of this, it is hardly

surprising that he never again produced anything of great significance after the Frederick book and that he was reluctant to the end of his life in 1963 to see it reprinted.

Eckhart Grünewald, in this doctoral thesis, touches on many interesting points but does not develop any of them very far. This is, he tells us, a preliminary sketch. There is little on Kantorowicz's life here, little in particular on the extravagance that was so characteristic of both his life and his intellect. This is a major failing because ideas and life were one and the same in this man. That Kantorowicz spent the money given to him by a friend for his escape from Germany in December 1938 on oysters is a fact of enormous symbolic value that should not be relegated, as it is in this study, to a footnote. Moreover, in any important work on members of the George circle, homosexuality, which was central to the group's aesthetic, must be discussed candidly. There is no mention of it in this essay. Nevertheless, despite these drawbacks, this study, based as it is on meticulous research, will assist greatly any future work on Kantorowicz or the George circle as a whole.

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DAVID WELCH. *Propaganda and the German Cinema, 1933–1945*. New York: Clarendon Press of Oxford University Press. 1983. Pp. xiv, 352. \$34.00.

For too long historians have overlooked the importance of film as a propaganda medium. From the first, Joseph Goebbels recognized that one cannot win the heart and soul of a nation through mass rallies and the printed word alone. Visual images brought National Socialist ideology to life and appealed to the emotional side of man's nature, thereby preparing millions of people to play a role in the struggle for Germany's domination of Europe. David Welch's *Propaganda and the German Cinema, 1933–1945* is an important work, one which provides a thorough analysis of the theory and use of Nazi film. An expanded thesis done under James Joll at the London School of Economics, the book establishes a secure place for the author among the distinguished corps of English film historians. It goes far beyond the works of Erwin Leiser, Richard Taylor, and David Hull, and compares favorably with the scholarship of Gerd Albrecht and Friedrich Kahlenberg. The book is firmly grounded in the sources and is based on important archival materials from the British Film Institute, the Institut für Filmkunde, and the Bundesarchiv.

The organization of the work is well suited to the task at hand. The book begins with an analysis of the *Gleichschaltung* of the film community, the formation of the *Reichsfilmkammer*, and the nationalization of the film industry. One chapter contains an excellent

discussion of the theoretical underpinnings of Goebbels's conception of film, which embraced at once aesthetic and ideological considerations. Subsequent chapters deal in turn with thematic areas such as "Comradeship, Heroism, and the Party," "Blood and Soil," "Führerprinzip," "War and the Military Image," and the "Image of the Enemy" (Bolsheviks, the English, and the Jews). Welch analyzes important films from the repertoire; illuminates the style, content, and the strengths and foibles of leading directors and film stars; and simultaneously sheds light on Goebbels's orchestration of the proceedings from conception to premiere.

The propaganda minister's road to achievement in cinema was difficult indeed. The year 1933 brought the first and last of the "party" films. Whereas *Hitler Youth Quex* (Herbert Norkus) was gloriously successful, the life of Horst Wessel—*Hans Westmar*—was flawed. The rhapsodic *Eternal Forest* (1936) was problematical, while *Olympiade* was a masterpiece. Although Goebbels found *I Accuse* (1941) aesthetically appealing, the public rejected its blatant message supporting euthanasia.

Welch demonstrates, as no author before him, that the historical films of the Third Reich had a contemporary message. His discussion of *Friedrich Schiller* (1940), *Bismarck* (1940), *The Dismissal* (1942), and *The Great King* (1942) is masterful, pointing throughout to the all-consuming figure of Hitler. The author is very adept at explaining how the Nazis used film to weave variations on their central ideological motifs. His depiction of the horrors of Bolshevik criminality in *GPU* (1942) is graphic, and he conveys the hysteria of propaganda directed against the English in a polished commentary on *Ohm Krüger* (1941). The section dealing with the notorious antisemitic films—*The Eternal Jew* (1940) and *Jud Süß* (1940)—is both sensitive and informative. A chapter on World War II moves from a learned analysis of the important weekly newsreel to the flamboyant *Kolberg*.

The book is not without its faults. There are far too many distracting quotations, stylistic infelicities, and misspelled words. Further, Welch needs deeper reading in the German cultural milieu. For example, one cannot include filmed classics of German literature such as Theodor Storm's *Der Schimmelreiter* (1934) in a "blood and soil" genre listing that includes such notorious Nazi productions as Carl Froelich's *Ich für Dich—Du für mich* (1934). Despite these criticisms, Welch is to be commended for this fine embodiment of his diligent research.

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CHRISTOPH GRAF. *Politische Polizei zwischen Demokratie und Diktatur: Die Entwicklung der preussischen polit-*

ischen Polizei vom Staatsschutzorgan der Weimarer Republik zum Geheimen Staatspolizeiamt des Dritten Reiches. Foreword by WALTER HOFER. (Einzelveröffentlichungen der Historischen Kommission zu Berlin, number 36.) Berlin: Colloquium. 1983. Pp. xvii, 457. DM 78.

Even democratic republics must defend themselves against their political enemies. Yet, if they are to preserve precisely that about themselves most worth defending, they cannot afford to deviate from those democratic norms that might inhibit the work of defense. This is the dilemma that often causes free citizens to worry about the executive branch of government in general and about political police—FBI, CID, *Sûreté*, *Bundesverfassungsschutz*, and others—in particular. In Weimar Germany matters were even worse, for it was not at all clear that the *Politische Polizei* (particularly in Prussia) adhered to even basic democratic values and professional norms. Nevertheless, if liberal and conservative commentators from K. D. Bracher to Juan Linz are correct in arguing that the Weimar Republic met its demise at least in part because it could not defend itself against its enemies, then one cannot simply dismiss the political police as inherently dysfunctional for democracy; but must, instead, probe deeply into the structure, personnel, and operation of that office.

Christoph Graf has done this in a most thorough and unprejudiced fashion, especially for the years 1930–34. His findings will provide no comfort for those who contend that the Nazis either marked a major break with what had preceded them in the realm of the police or that German conservative authoritarianism opposed Nazi efforts to gain and transform power. Neither was the case. Nor will his findings encourage those who have come to see the Nazi regime as somewhat chaotic, without clear plans, and given to radicalization as a substitute for problem solving. Where the regime was heading was, in Graf's view, clear from the outset, well before Reinhard Heydrich's and Heinrich Himmler's transformation of the political police (via the secret police bureau) into the Gestapo. These two central aspects of Graf's argument are linked by his persuasive presentation of 1932 as the key year.

Despite their democratic commitments, the Socialists in charge of the Prussian executive after 1919 found themselves engaged in reestablishing political and social order. Like the Reich government more generally, they retained and recruited unrepentantly antirepublican cadres with very little, if any, loyalty to their nominal superiors. Many of those men were, for Graf, sincere *Rechtstaatler*. In spite of this, they found action against Communists much easier and more natural than action against Nazis: Communists were class as well as political

enemies, whereas the Nazis observed legalisms and often shared class and cultural backgrounds with the political police. Where the Socialists stood in the eyes of the political police before 1932, Graf does not make sufficiently clear, given what happened in mid-1932.

Perhaps it was only change at the top that really mattered, and that certainly came with Franz von Papen's putsch against the Socialist government of Prussia in July 1932. This lies centerstage in Graf's analysis, and here he makes his most powerful arguments. With Papen's move the political police became "a completely one-sided instrument of struggle against communism and loyal and formerly governing social democracy while National Socialism, in accord with Papen's dependence on Hitler and despite occasional differences between the two, became both legalized and respectable" (p. 437). Cooperation between the political police and the Nazi party grew, so that the continuity of institutions, personnel, and policies between the Papen regime and the Third Reich was nearly complete. Graf's presentation makes clear just how mistaken H. A. Turner's argument is that Papen's government represented an attempt "to stave off the Nazis." On the contrary, at least as far as the political police were concerned, January 30, 1933 was but the logical continuation of July 20, 1932.

Not only did the personnel installed by Papen contribute "directly and substantially" to the Nazi takeover, but the Nazis subsequently felt no need to purge the political police in the course of their "legal revolution." Contrary to his subsequent apologetics, Rudolf Diels, head of the political police/secret police bureau from July 1932 to April 1934, worked hand in hand with the Nazi party and the SS. Further, virtually all the goals, methods, and victims of the repressive apparatus emerged clearly in the years 1932-34, leading Graf to conclude that the Nazi regime was from the outset a totalitarian dictatorship. Personal as well as administrative rivalries surely existed, but the police and the regime as a whole pursued clear goals in a directed manner. Graf takes clear and often persuasive exception with those scholars who have come to see "pluralistic" chaos as the regime's motor force. One might object, in defense of that school, that Graf's conclusion is biased by the object of his study—if any agency is totalitarian, it is surely the political police—but the evidence he provides is powerful nevertheless. Scholars venturing into that debate will have to confront Graf's findings and interpretations.

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KLAUS KINNER. *Marxistische deutsche Geschichtswissenschaft, 1917 bis 1933: Geschichte und Politik im Kampf*

der KPD. (Akademie der Wissenschaften der DDR, Schriften des Zentralinstituts für Geschichte, number 58.) Berlin: Akademie. 1982. Pp. 526. DM 48.

The use of the term *Geschichtswissenschaft* in the title is misleading for the Western reader. *Geschichtswissenschaft* generally refers to academic historical scholarship. Here "science" and political "praxis" are interwoven. The subject matter of Klaus Kinner's book is the interpretation of the European past by "communist functionaries, theoreticians, and propagandists" (p. 193) as an integral part of the "ideological struggle of the party of the working class" (p. 11). The sources extend from party resolutions and the "speeches and writings of leading functionaries of the KPD and the Comintern" (p. 10) to newspaper articles and posters. The scientific character of Marxism or, better said, Marxism-Leninism consists in the recognition of the lawful character (*Gesetzmässigkeit*) of the transformation by which the various "social formations" succeed each other in the process of achieving a communist society. Marxism has come to mean various things in the course of the twentieth century. Here it is identified with the "objectivity" (p. 517) and necessity of the historical process as reflected in Lenin's writings. The attempts by Karl Korsch and Georg Lukács in the 1920s to stress the role of cultural factors, according to Kinner, led to an "eclectic mixture of Marxist and bourgeois conceptions" (p. 32) that need to be rejected.

Marxist *Geschichtswissenschaft*, Kinner maintains, coincides with the history of the Soviet Union and the Communist parties. The crucial caesura is the October Revolution of 1917. The book is divided into three sections, the formative period of the German Communist party from 1917 to 1923, the period of ideological and political consolidation under the leadership of Ernst Thälmann from 1924 to 1929 (the period that, incidentally, coincides with the Stalinization of the party), and the period of struggle against fascism from 1929 to 1933. The author prefers not to speak of the influence of Stalin on the German party. Each of the three sections analyzes the party's views of the great revolutions that were necessary steppingstones in the forward march of socialism: the Reformation and the German Peasant's War are seen in Engels's terms as an "early bourgeois revolution"; the French Revolution, uncritically as the "model case of a bourgeois revolution;" the revolutions of 1848 and 1849 as the first "bourgeois revolution" on a universal scale; and the Paris Commune as the first "proletarian" revolution. The history of the German worker's movement in terms of the Social Democratic party (SPD) and the early Communist party (KPD) and, most important, the history of the Bolsheviks complete this study of revolutionary movements. These revolu-

tions and revolutionary movements form a necessary chain in the victory of the working class.

Historical science, however, means something more in the German Democratic Republic in the 1980s than it did for the KPD in the Weimar Republic. Historical studies in the former continue to possess a direct political function. The resolutions of the party congresses continue to be decisive. Kinner's book is useful for an understanding of the ideological context in which the history of the modern world is conceived in the German Democratic Republic. Nevertheless, the study of history has undergone a process of professionalization since 1945. Professionalization, in turn, has brought with it a reliance on methods of critical scholarship that are shared by historical scholars generally. Without breaking the tie with ideology, the history study has increasingly been based on scholarly foundations of research that make a fruitful discussion between Marxist-Leninist and non-Marxist historians possible.

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MICHAEL H. KATER. *The Nazi Party: A Social Profile of Members and Leaders, 1919–1945*. Cambridge: Harvard University Press. 1983. Pp. xiv, 415. \$25.00.

This book provides the most extensive data-based portrait of Nazi party members and leadership ever assembled. Michael H. Kater has sampled national membership lists and has brought together every local party listing that ever surfaced. For this "mining" effort and generation of raw materials, he deserves very high praise.

The work is divided into two parts, one dealing with the rank and file, the other with leadership cadres. The key chapters provide statistical portraits plus qualitative analyses of the causal factors at work in the various periods of the party's twenty-six-year history. Two chapters provide social profiles of the followers and leaders. There is a brief conclusion.

The chapters on the rank and file describe the occupational composition of the members, specifically of those joining in each of five periods. These figures are contrasted with statistics on the occupational composition of the entire nation, thus allowing an assessment of the over- or underrepresentation for the three classes Kater has delineated—the lower class (workers), the lower middle class, and the elite. He then analyzes the situation of the classes during the period in question to account for the reported tendencies.

Serious difficulties arise with every step of Kater's analysis. The occupations of the members are contrasted with those of the entire labor force. But the

party, as Kater's social profile shows, was overwhelmingly male in its composition, which would argue for separate analysis by sex. The 95 percent or so male membership should have been compared only with the male labor force. Since males and females, then as now, have rather different patterns of employment, his statistical conclusions are open to considerable question. While it seems unlikely that the basic class pattern would be eliminated, reversed, or even accentuated, for the moment one must see all of these findings as tentative rather than definitive.

It would have been helpful if Kater had made use of some index figure to allow easy summary of his results. As it is, the reader is referred, in each case, to the appropriate tables at the back of the book, then must assemble figures and either judge or make calculations to discover the extent of the bias involved. The principal finding, probably the most important one in the entire book, is that the involvement varies directly with class level. That workers were modestly underrepresented in the party is nothing new. That the lower middle class was overrepresented will also come as no surprise, perhaps only the modest degree of the bias being unexpected. The most important finding is the extent of elite overrepresentation, something not anticipated in most other works.

This last finding seems to bother Kater. He certainly does not give it the attention it deserves. Instead, he focuses on another statistic, the absolute number of lower-middle-class members—they unquestionably were the majority in the party—and dwells on their presumed dominance in party affairs. But such dominance would follow only for a democratically run organization, a rather inappropriate assumption in this case.

A problem arises with Kater's class divisions and his use thereof. His "lower middle class" forms 43 percent of the labor force (as against a tiny "elite" of less than 3 percent). This large class includes the self-employed farm population plus nearly all of the urban nonmanual category. The latter thus embraces those persons on the lower margins of the middle class (those ordinarily said to have provided the Nazi social base) plus all others below the top management level (managers and entrepreneurs, by Kater's reckoning, form less than 1 percent of the total). That large sweep is, in fact, indicated in table 1 where the two relevant categories are labeled "lower and intermediate employees" and "lower and intermediate civil servants." Kater seems troubled by the implication here, since, in both cases, after the word "intermediate" he has, in parentheses, inserted the word "petty." Elsewhere, the two categories are referred to simply as *lower* employees and civil servants. The reader of the book must keep in mind that Kater's "lower middle class" includes a category

that most people would call the upper middle class, a category that is not adequately described as petty bourgeois.

Key questions arise in this connection: how do the upper- and lower-middle-class segments behave? Is the lower middle class overrepresented, as most would assume? Or is there, paralleling the elite pattern, upper-middle-class overrepresentation? These questions are not adequately treated.

The six subgroups of Kater's "lower middle class" show considerable variation in tendency. One might anticipate the master craftsmen (9.65 percent of the labor force) to be in the "lower-middle" segment. If so, the results are rather equivocal for the received thesis. Table 2 shows that for the period 1919–23 the craftsmen are overrepresented in seven cases, underrepresented in seven, and roughly equal in the last. This pattern is reflected in tables covering the other periods as well.

Some of those statistics are for towns and cities, while others provide national occupational figures. Therefore, the comparisons are not appropriate. The absence of farmers among town and city members means that all other categories are proportionally higher. This could easily lead to the mistaken conclusion of overrepresentation of master craftsmen in the party lists when the comparison is with national labor force figures. The data for 1930–32 show a considerable overrepresentation among nonacademic professionals, merchants, and farmers. There is a small overrepresentation of master craftsmen; the employees and civil servants are both slightly underrepresented. To answer the key questions, one must disaggregate the upper and lower segments in each of these categories. Until that is done, the question of the tendencies of those segments must remain open; there is no justification for any unambiguous conclusion about the lower middle class. The case of the master craftsmen may provide us the best, albeit limited, clue to what that disaggregation would show; it suggests overrepresentation of the upper-middle-class segments.

Kater's analyses of his findings also involve some serious failings. For each period, he describes the condition of the respective classes as a whole. But that is misleading. It is not the entire class that is responding, but a tiny fraction of it. The analysis, therefore, should deal with the characteristics, circumstances, and motives of that tiny minority. The overwhelming majority of those classes did not join the party at any time. The key question of internal differentiation of the classes is consistently missed.

The explanations of the findings constitute the bulk of the principal chapters. The arguments are supported by a vast assemblage of footnotes. On the basis of a limited sampling, however, that support proves to be less than compelling. The secondary

sources cited, in many instances, do not provide additional evidence in support of the text. One finds instead merely another testimony; someone else, it turns out, has said the same thing. But no evidence is contained in these sources on the claims at issue, just proof that the author and others agreed about something. To the best of my knowledge, Kater's claims about a "lower middle class mentality" have no solid empirical support anywhere in the entire social science literature.

If the example of the craftsmen is symptomatic of the pattern within Kater's broad middle class, it would mean his basic finding is that the higher the class level, the greater the National Socialist involvement. One must, however, ask a further question: would that result be substantially different for any other nonleftist party? If not, what is the significance of Kater's finding? This relates to the previous point that his explanatory analysis should have been focused on internal differentiation: the basic questions should have been why some segments of the elite, the lower middle class, and the working class went to the Nazis; why some made their choices in other directions; and why some, perhaps the vast majority, joined no party.

It is with much regret that one finds it necessary to be so negative about this book. As indicated at the outset, Kater has done a formidable job of data collection. It is unfortunate, however, that the analysis of those materials has not been commensurate with the vast effort. This book is not "the definitive study," as one commentator has described it. It contains the materials for a definitive study, but the heart of that analysis still remains to be done.

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CONAN FISCHER. *Stormtroopers: A Social, Economic, and Ideological Analysis, 1929–35*. Boston: George Allen and Unwin. 1983. Pp. xiv, 239. \$25.00.

This study of the *Sturmabteilung* is a significant addition to the literature on the social background of the National Socialist movement. Conan Fischer has combed through documents in national and regional archives to support his thesis that the majority of rank-and-file SA members, both before and after the Nazi seizure of power, were working class in background. Young, unemployed workers were the most representative group. While this is not a new thesis, Fischer's work offers more conclusive evidence to support it.

The authors of past studies have erred by failing to separate the SA's largely middle-class leadership from the rank and file when analyzing SA membership. Yet, the SA's middle-class leaders also furthered the paramilitary organization's proletarian character by their support of populist programs and

through their rhetoric. By seeking to lighten the economic burdens on the membership through welfare assistance and unemployment aid, the SA leadership increased the organization's attractions for workers.

Fischer's research demonstrates that the SA constituted a distinct and quasi-independent element within the Nazi movement. Often based in working-class neighborhoods, the stormtroopers were a natural rival of the Communist Red Front and the Social Democratic *Reichsbanner*. The movement of members between the Red Front and the SA was one aspect of this. Ideology was clearly less important than the attraction of extreme means and programs as a way out of the Weimar system and the economic crisis. After the Nazi seizure of power, membership in the SA represented a means for workers to accommodate themselves to the regime and to better their economic situation. But the unwillingness of the Nazi government to place any priority on the stormtroopers' plight was a prime cause of the growing unrest within the SA during the first half of 1934.

Fischer argues that the SA rank and file, before the Röhm purge, were never ideologically integrated into the Nazi movement. The SA leadership's populism, which was supported by the membership, also differed markedly from the party leadership's program that sought accommodation with the army, civil service, and industry after the takeover. During the *Kampfzeit* and the terror of 1933, the SA's radicalism and street-fighting tactics aided the party's struggle for power. But after the violent spring of 1933, the stormtroopers lost their usefulness, and the quasi-independence of the SA threatened to undercut the Nazi leaders' goals. The Röhm purge and the subsequent restructuring of the SA were logical outcomes of the SA's relationship to the party.

Fischer's monograph effectively relates quantitative findings to individual case histories and topical analysis. He adds an important dimension to our understanding of the complexity of the Nazi movement. While much of the book will primarily interest scholars, chapters dealing with the unemployment question, financial and welfare assistance, and ideology and politics deserve a wider audience. The Nazi movement's ability to harness the inchoate radicalism of significant numbers of the unemployed has wide significance for our understanding of the political perils of economic crises.

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JOHNPETER HORST GRILL. *The Nazi Movement in Baden, 1920–1945*. Chapel Hill: University of North Carolina Press. 1983. Pp. xv, 720. \$32.00.

A number of regional studies of the rise of Nazism have appeared during the past decade but few cover the years of the Third Reich itself. For this reason, Johnpeter Horst Grill's decision to study the role of the Nazi party in Baden has much to commend it, particularly since Baden has a number of features that make it peculiarly appropriate for such a study. The sources—particularly for the various Nazi organizations—are unusually rich, and Baden's socioeconomic structure and pattern of religious adherence were both similar to those of Germany as a whole. Their diversity permits comparisons between the responses of different socioeconomic and religious segments of the population. In addition, during the war, the Nazi Gauleiter of Baden was appointed to head the German occupation in neighboring Alsace. Grill's study focuses on how and why the Nazi party was able to capture a substantial percentage of Baden's voters in 1932, on how the population reacted to Nazi rule after 1933, and on the importance of the Gau party in the seizure and maintenance of power by the Nazis.

To answer these questions, the author makes very thorough use of the rich original source materials available. He provides a very solid account of the rise of Nazism in Baden that, however, largely confirms the picture formed by studies of other areas. Clearly, future area studies of the rise of Nazism will have to focus on in-depth analyses of much smaller localities in order to come up with fundamentally new insights. On the question of the role of the party in Baden after 1933, Grill provides much interesting material. He demonstrates clearly that, although totalitarian in aspiration, the party was by no means monolithic in practice, having disintegrated into its component parts—party affiliates and organizations that were permanently at war with one another over spheres of influence. The cadre organization endeavored to maintain coherence. It too, however, was by no means a cohesive structure, bedeviled as it was by fluctuations of personnel and conflicts between the various levels. At the same time, Grill argues that the pluralism of the party affiliates "strengthened the Nazi rulers' control of German society by isolating and fragmenting Baden's socio-economic interests." Moreover, the cadre organization at the Gau level—the clique of old associates of the Gauleiter—did provide a relatively stable group "since they were not bound by any socio-economic identification or connected with any specific interest" other than the party organization, and "provided a key element in the Nazi system of government." These aspects, too, although largely confirming previous research, are well brought out. Finally, there is a useful chapter on the party's role in Alsace.

The main weakness of the book, and it is a serious one, is that the author totally ignores virtually all the

work that has appeared in Germany on this topic during the past five years or more. The seminal studies emerging from the Bavarian project of the Institut für Zeitgeschichte, Martin Broszat's standard work, *Der Staat Hitlers* (1969), T. W. Mason's work on labor and society, for example, are all ignored. This is a pity because, with the rich sources that he has used, Grill could have made a useful contribution to current historical debates on a number of issues.

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ROLAND G. FOERSTER *et al.* *Anfänge westdeutscher Sicherheitspolitik, 1945–1956*. Volume 1, *Von der Kapitulation bis zum Plevén-Plan*. Munich: R. Oldenbourg. 1982. Pp. xxv, 940. DM 88.

This is the first of a projected three-volume study of West German rearmament published under the direction of the *Militärgeschichtliches Forschungsamt* in Freiburg. Subsequent volumes will deal with the debate over the European Defense Community (EDC) and the formation of the Bundeswehr as a part of the North Atlantic Treaty Organization (NATO). Volume 1 consists of six extended essays by five scholars. Although not without some of the defects of a team project—occasional unevenness, overlaps, redundancies—the book possesses the broadly defined view of military history and the high level of scholarship we have come to associate with the *Forschungsamt's* publications. Although the older works by Gerhard Wettig and Arnulf Baring will continue to be useful, this project, when completed, should become the definitive study.

The first three essays describe the international context of the decision to rearm West Germany. Norbert Wiggershaus describes the growing rifts in the wartime anti-Hitler coalition, the confrontation of the superpowers in Germany, and the formation of the Eastern and Western blocs. This is followed by a detailed investigation by Christian Greiner of Allied military planning for the defense of Western Europe from the onset of the Cold War until 1950. In the third essay, Wiggershaus recounts the decision of the Western Allies for a German defense contribution that eventually led to the Plevén Plan. Wiggershaus and Greiner do an admirable job of synthesizing a vast amount of material, but their conclusions are not startling. Both emphasize the importance of the Korean War. Whereas U.S. participation in the Atlantic Pact had been limited by budgetary constraints and the belief that its nuclear superiority would deter aggression, after June 1950 the U.S. acted to provide the levels of support that the European allies hitherto had been insisting on in vain. The decision to strengthen conventional forces

in Europe, while limiting costs and the direct employment of U.S. troops, led almost inevitably to the consideration of using German troops, at least in the minds of the British and Americans. In this the interests of the Anglo-Americans converged with those of the new West German chancellor, Konrad Adenauer, who, as Wiggershaus shows, hoped to use rearmament as a lever to pry concessions from the occupying powers. The decision to rearm West Germany raised problems, however, not only with the French but also in Germany.

The focus of the second half of the book accordingly shifts to the domestic political arena. Roland G. Foerster traces Adenauer's rearmament policy and explores its domestic ramifications. Like Wiggershaus, Foerster emphasizes that Adenauer used rearmament as a means to acquire greater sovereignty. The chancellor's efforts were initially hampered by the disarmament policies of the Allies and the indifference or outright hostility of large segments of the German population. The former were soon removed, but the latter remained—reeducation, as the Allies were to learn to their dismay, had succeeded all too well. As a consequence, Adenauer was forced to pursue his rearmament initiatives with great caution, an experience, Foerster suggests, that may have encouraged the secretive, often devious, and high-handed tactics that generally characterized his policy. While Adenauer's party and even cabinet members were often kept in the dark regarding the chancellor's rearmament plans, leaders of the opposition Social Democratic party (SPD) were not. This is a significant difference between the two postwar periods and a major reason why Bonn is not Weimar. Both Adenauer and his military advisers were adamant that bipartisan support, at least in matters of principle, was imperative to construct a viable defense policy. The SPD reciprocated by adopting a policy of responsible opposition. Consultation reduced mutual suspicion and produced a number of benefits, perhaps the most important being the firm integration of both the SPD and the military into the new state.

The final two essays of the book deal with questions raised by the decision to rearm: who would lead the new army and what sort of army would it be? As Georg Meyer shows, a solution to the first question proved difficult, for the *ohne mich* attitude extended beyond potential conscripts into the ranks of the former Wehrmacht officers who were expected to be the leaders of the new army. Angered by the antimilitarist measures imposed by the Allies after the war, former officers either refused outright to serve again or, more often, coupled future participation with the fulfillment of demands, foremost among which were an end to the "defamation" of the military and the restoration of abrogated pension rights. Meyer carefully describes the origins

of the former officers' grievances. While his generally sympathetic account provides a useful corrective to conventional antimilitary polemics, his conclusion, that in view of their suffering it is "astounding" that former officers were prepared to participate in the building of a new German army, overshoots the mark. As he himself shows, the officer corps of the Wehrmacht was not as monolithic as its detractors claimed; and by addressing their legitimate grievances, the government was able to isolate the ultras and to attract the more moderate elements.

Strict civilian control and the successful recruitment of reform-minded military planners permitted the creation of a reformed German army. The activities of the reformers are traced by Hans-Jürgen Rautenberg, who notes that military reform was an essential precondition of rearmament. The simultaneous collapse of the German army and the German Reich made it necessary to redefine both Germany's place in Europe and the military's role in Germany. The new German army was not to be a national army, but a contingent of an European army, a radical departure from past practice in which military independence was considered the *sine qua non* of national sovereignty. This fundamental change in international status was accompanied by equally fundamental changes in the internal structure and ethos of the new German army. While the bureaucracy, school and justice systems, economic structure, and political parties were subjected to some corrective reforms, Rautenberg argues, they were, on balance, marked more by lines of continuity to their (non-Nazi) pasts than by substantive new departures. Whereas other institutions frequently oriented themselves by reference to the Weimar Republic, even this was denied to the military because of the Reichswehr's open complicity in the republic's fall. Not without irony, Rautenberg observes how representatives of often only imperfectly reformed bodies now insisted that the military be thoroughly reformed.

In the end, the close, if at times hypocritical, scrutiny of German rearmament produced positive results: the Bundeswehr emerged as one of the more progressive armed forces in the world. Indeed, during the EDC discussions, Allied military officials were at times appalled by the far-reaching reforms proposed by the Germans and refused to introduce them into their own forces. In NATO it was not the Germans who would play the Prussian role.

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GREGORY W. SANDFORD. *From Hitler to Ulbricht: The Communist Reconstruction of East Germany, 1945-46.*

Princeton: Princeton University Press. 1983. Pp. xiv, 313. \$25.00.

Within little more than a year after the Nazi surrender in May 1945, German Communists transformed Soviet-occupied Germany into a "democratic people's republic." Supported by many non-Communists, they confiscated all large estates and parceled them out to landless and land-poor peasants or turned them into state property. They expropriated and nationalized major industries, put their management under workers' supervision, imposed wage and price controls, and resorted to central economic planning. The Communists established a coalition of political parties dominated by their own party, purged and reconstituted the civil administration, and set up various new "organs of the people"—trade unions, mutual aid societies and cooperatives, chambers of commerce and industry—in which they held key positions. By the summer of 1946, the old ruling classes were gone, and their property and power were in the hands of "progressive" elements led by the Communists.

This profound transformation has been interpreted variously. Some see it primarily as an effort to denazify and democratize German society in order to prevent a resurgence of fascism and create a state well disposed toward the Soviet Union. Others, mindful of East Germany's subsequent history, see it as the deliberate first step toward a Stalinist dictatorship in the Soviet Zone.

Gregory W. Sandford argues that the two interpretations need not be mutually exclusive. In a careful, well-presented account of the events of 1945-46, he shows that the German Communists, working closely with the Soviet Military Administration and prompted both by ideological perspectives and by practical considerations, chose a path of compromise. They did not impose Communism; rather, they allied themselves with left and center parties to foster "national regeneration" and promote a "bourgeois democratic republic," part capitalist and part socialist. They made concessions to elicit cooperation, tolerated some forms of dissent and diversity, and, although they never denied that they intended ultimately to introduce a socialist system, pushed their program of reform and dominance no further than seemed necessary to assure the destruction of the old order. Ideologically, such a course was justified by the argument that the situation in postwar Germany corresponded to that in Russia after 1905 and that the Germans, in keeping with their materialist destiny, would now have to complete their bourgeois-democratic revolution. As a practical matter, such a course secured maximum leverage for the Soviets: it prejudiced the future of East Germany even while it preserved good relations with the wartime allies. When these

cautious tactics were abandoned in 1949, the Communists could erect their state on political, social, and economic institutions they controlled.

Sandford's study is based in part on research in East German archives not previously open to Western scholars, and it adds much new detail to what has been known in broad outline. With its focus on social and economic developments, it complements Henry Krisch's *German Politics under Soviet Occupation* (1974), although, unlike Krisch's study, this work tends on occasion to overestimate ideological motivations and to invest the course of events with a kind of logical inevitability, leaving little room for the unexpected that often shapes history more than the best-laid plans.

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MICHAEL HEYD. *Between Orthodoxy and the Enlightenment: Jean-Robert Chouet and the Introduction of Cartesian Science in the Academy of Geneva*. (International Archives of the History of Ideas, number 96.) Boston: Martinus Nijhoff or Magnes Press, Hebrew University, Jerusalem. 1982. Pp. xii, 308.

Michael Heyd's book focuses on the Genevan Academy, an institution whose European importance was declining during the seventeenth century, and on Jean-Robert Chouet, a not very innovative thinker. Nevertheless it is an important study of the process of seventeenth-century intellectual change, of the role played by educational institutions in diffusing new ideas, and of the complex relationship between religion and science. Because this relationship typically has been examined in connection with the more empirical tradition of English science and its connections to Puritanism or latitudinarian Anglicanism, this study provides an excellent opportunity to reexamine science-religion issues from a comparative perspective.

The first and most important portion of the book examines how Cartesian philosophy was introduced into an orthodox academy whose primary function was to train Calvinist ministers. The key role in this process was played by Chouet, who initially adopted Cartesianism to buttress orthodox Calvinist theology. His insistence on the separation of theology and philosophy proved critical, for it meant that religious orthodoxy could be maintained as Cartesianism was introduced into the philosophy course. Yet Chouet's teachers and predecessors had prepared the way, adopting "half way houses" between Aristotelianism and Chouet's full-fledged Cartesianism. Chouet tactfully combined scholastic terminology and method with a Cartesian content, thus insuring

that the new philosophy and physics were introduced gradually and without ideological or methodological conflict.

Like many Cartesians of his generation, Chouet soon abandoned the deductive science originally envisioned by Descartes for the probabilistic reasoning and experimentalism of the later Cartesians. Heyd's description of the gradual modification of Aristotelianism and the varieties of Cartesian thought portrays the complexity of seventeenth-century intellectual life.

The academy continued to change after Chouet's departure. The influence of the Genevan magistracy in curricular matters and structural reform developed rapidly. Once the new philosophy had been legitimized and accepted, it began to affect theology and to influence the entire curriculum. Steps were taken to harmonize religion and science and to adopt a non-Calvinist rational latitudinarianism. Once scientific knowledge and method were officially integrated with theology, the curriculum was substantially revised. Philosophy was no longer based on dialectic but on a mechanistic physics, and mathematics displaced logic as the preferred form of training for the mind. Cartesianism now became an ideology that presented natural science as the model of human knowledge.

In less than a century the goal of the academy shifted from the training of a Calvinist ministry to the education of the "honnête homme." The academy emerged as an "enlightened" institution characterized by a modified Cartesianism and a rationalized latitudinarian theology. As it became more secular, modern, and "enlightened," however, it lost its international clientele and became a more local institution.

To some extent the picture Heyd gives us of the Genevans shows them following in the theological footsteps of the English. In England the Baconian separation of religion from natural science was rapidly followed by an effort to harmonize the new philosophy with a non-Calvinist rational theology. The small size and increasing lay control of the Genevan Academy meant, however, that fundamental curricular change occurred far more rapidly and easily than in England's old and relatively autonomous universities. Although some scientific teaching and research occurred within the English universities, there was no equivalent sustained effort by lay authorities to reform radically the curriculum at Oxford and Cambridge.

The latter portion of this book employs a statistically based social history in an attempt to uncover the social causes and conditions of the intellectual dynamics charted in the earlier position. Heyd's thoughtful and carefully researched study treats important issues of intellectual change and its causes thoroughly and sensitively. Certainly it should be

read by all those interested in the diffusion of the new science and the origins of the Enlightenment.

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RICHARD C. TREXLER. *Public Life in Renaissance Florence*. (Studies in Social Discontinuity.) New York: Academic. 1980. Pp. xxv, 591. \$45.00.

By "public life" Richard C. Trexler means "the way in which Florentines from Dante to Machiavelli interacted with one another, with foreigners, and with their divinities" (p. xiii). In this long, bold, dense, and complex book he undertakes to analyze the ritual and ceremonial forms in which those actions took place, demonstrate the meanings and purposes of formal behavior, and furnish a new interpretation of Florentine republican history. *Public Life in Renaissance Florence* is the product of many years' study in the Florentine archives as well as broad reading in the literature of anthropology, comparative religion, and sociology. In applying the concepts of these fields to this Renaissance city *par excellence*, Trexler is rightly conscious of having done something innovative for Florentine history and, perhaps, for social science as well.

Trexler presents a vivid picture of the Florentines worshipping, walking together in procession, manipulating their sacred images, defining and redefining their public spaces, elaborating and revising their civil and religious ceremonies (he denies any important distinction between these two categories), and watching each other do these things. By peering through the lens of ritual at the events and institutions of Florentine history, he has viewed a different Florence, not unfamiliar to us in its individual features but startlingly different in its entirety. We have seen the countless representations of and references to saints and relics in Florentine painting; we know that viewing sacred icons was a popular activity; we already know that the Medici consciously used symbols to project the desired images of themselves; we have read about Savonarola's transformation of carnival behavior into devout processions. But we have not before been asked to consider that this was all of a piece, a fundamental mode of expression crucial to the life of the city. Trexler weaves the ritual forms of Florentine life into a civic, mainly nonverbal language that communicated assessments of love, hate, honor, power, and divine favor and through which Florentines found and expressed their individual and collective identities vis-à-vis each other, foreigners, and their gods. Using this language as a heuristic tool, he describes the biological, social, political, and religious contexts of Florentine interactions, and he sees changing

ceremonial and ritual patterns as a key to understanding the transformations and, ultimately, the fall of the commune.

At times this approach is brilliantly illuminating. Trexler's formalist analysis of the famous friendship between the notary Ser Lapo Mazzei and the rich merchant Francesco Datini highlights the delicate interplay of sentiment, sincerity, self-interest, and mutual obligation in Florentine society and makes a contribution to the history of friendship. His dissection of diplomatic ceremonial behavior beginning with the entry of the visitor and his formal tour of the city's shrines and orientation to its public spaces helps us to understand the religious and political significance of what has usually been dismissed as mere pomp or empty display. His analysis of Medici manipulation, first of republican and then of princely symbols, adds an important dimension to our understanding of the methods by which Lorenzo the Magnificent and his sixteenth-century successors first transformed and then eventually killed the commune. As far as I know, Trexler is the first person to take seriously the religious play acting in which Savonarola and Saint Caterina de' Ricci engaged with their fellow friars and sisters. His interpretation of these "diversions" (as they were habitually called) illuminates a little-known side of convent life.

Trexler's analysis is both diachronic and synchronic. He offers a new periodization of Florentine history according to "behavioral epochs," chronicling the changing rituals by which first one social group then another moved from the margin to the center. We see the fourteenth-century commune gradually overcoming its "ritual isolation" by creating new, powerful, charismatic symbols and, in effect, "seizing the streets" from the "courts and castles" of the genteel families who had dominated the early commune and whose ritual forms were now relegated to subordinate and occasional roles. We see the "liminal" workers first brought into the civic feasts by Walter of Brienne and thrust out again at his expulsion by the victorious patriciate in 1343. The feast of Florence's patron John the Baptist was the main battleground, with subaltern groups struggling for ritual identity by participating in the feast against the exclusionary policies of the oligarchic Guelf party. We watch as the workers of the Ciompi briefly win their right to organize and participate in government in 1378 by seizing the civic banners and consolidating their new identity and legitimacy through "premodern urban ritual." The struggle of neglected groups to penetrate the center continued for decades, but by the 1470s a "processional revolution" was in the making. Not only workers, now organized in neighborhood *potenze*, emerged but also confraternities of boys and societies of *giovani*, the young adult males who now

began to claim a salvational role in the ranks of the public processions, creating "new sacred images, times, and places." The Medici, particularly Lorenzo the Magnificent, abetted these trends, thinking to direct these untapped energies and loyalties to themselves as the new ritual center of the commune. Savonarola took the ritual revolution further, replacing the clergy with angelic children at the head of his processions; but the neglected *giovani* desecrated his processional crosses and precipitated the "ritual crisis" that necessitated the disastrous and aborted trial by fire. Not until the desperate days of the last republic (1527–30) did the Florentines turn for ritual and military salvation to their *giovani*. They even made an effort to bring in women, but by then it was too late. Under the protective lances of Charles V's troops, the Medici no longer needed to manipulate republican symbols or cooperate with liminal groups. In the new absolute state civic processions became princely retinues and civic saviors passive spectators.

Trexler means to show that the Weberian legacy of regarding the city as "antiritualistic and disenchanting" has been pernicious both in leading to exaggeration of the rationalism, individualism, critical distancing, and self-conscious separation of subject from object of the Renaissance mentality and in repressing an important part of the human heritage, that is, "humanity's need to congregate" (p. xviii). He wants to demonstrate that "premodern urban ritual was an important means of creating, maintaining, and transforming life" (p. xxi), and he usually adopts the stance, at least with respect to religion, that it was indeed the primary means, far more important than doctrine or belief systems, in determining action. According to Trexler, ritual was particularly creative in Florence, that "base city on the make" (p. 313). Insecure about the inadequacies of their burgher culture, their usurious livelihoods, and the juridical illegitimacy of their upstart commune, Florentines used relics and ritual to sacralize their profane time and space, endow their crass business and political exchanges with trust, authenticate their government by drawing in honor from foreign feudalities and princes, and maintain stability and tradition. According to Trexler, the official belief structure was largely irrelevant to these purposes. Florentines might subscribe to the Christian teaching that sacred objects had power from God either intrinsically or through priestly consecration, but their behavior shows that Florentine laymen were engaged in creating, retaining, and destroying holy power.

Florentine religion, then, and a large part of public behavior, essentially consisted of sustaining life by collecting and reconstructing fragments of relics (the Osirian myth serves Trexler here) and of miraculously creating and transforming life by

manipulating images. Theologians and preachers might pound home the difference between reverence (*dulia*) and worship (*latria*) and between image and its referent (Trexler seems to conflate these two separate problems), but to no avail. To the Florentines, Mary was her image and the image was the power. The image saw, bled, felt anger and joy; but it was the Florentines themselves who, through devotional gestures and formulas they learned from infancy, knew how to frame, honor, manipulate, and evoke the power of the image. In sum, they made their divinities. From this perspective it is easy to see how religion and politics intersected. Since the holy was there where the ranks of citizens formed, the commune's task was to allocate and regulate access to honor and to the sacred shrines. "Images, relics, and hosts . . . functioned . . . as sacred bridges tying and retying persons and commune to the sources of outside authority. Civil communication was, in short, ceremonial and sacrificial to the extent that it was successful" (p. 127).

It would be patronizing to regard Trexler's extreme formulation of his thesis as the kind of rhetorical overstatement we may expect to encounter in an argument so self-consciously revisionist. It is true that on page 552 he states that his ignoring "political and religious dogma" is a strategy and that the question of "the relation between ideas and actions" remains to be studied; but having just been beaten about the head and shoulders for 551 pages with his thesis that systems of belief had little relevance for Florentine religion or public behavior, readers are not likely to consider this much of a concession. They will rightly conclude that the argument is reductionist.

To support his thesis that "a religion is a system of reverential behavior shared by a sworn community" and "we deceive ourselves to think of it as a community of belief" (p. xviii) he must show that the Florentines too deceived themselves, since many of them seem to have assumed religion to be just that—a community of belief. This he does by disallowing what Florentines *thought* they believed and *thought* they were doing, explaining to us instead what they were really up to, that is, making divinities and, ultimately, worshipping themselves (p. 94). This is neither good social science nor good history. There is a sense in which almost everyone, believer and nonbeliever alike, can agree that people make gods in their own image. Yet the scholar's job is not merely to elaborate on such truisms but to try to understand how people manage to reconcile these enduring impulses and needs with increasingly sophisticated and complex systems of belief and value. If some, most, or all Florentines acted on the belief that they worshipped a transcendental God and merely venerated images that merely represented saints, it behooves the historian to take that convic-

tion seriously, not to relegate it to some secondary order of reality.

Florence, as generations of scholars have established beyond question, was a culture in which linguistic sophistication, subtle reflection, and objective calculation were of the essence. As Trexler has shown, these qualities have obscured other facets of the Renaissance mentality to historians still under the influence of Burckhardt and of assumptions about rationality and positivism. In analyzing the ritual, nonverbal components of that culture, Trexler has made an important contribution, but his urge to destroy the received picture has prevented him from achieving the reintegration of thought and behavior that the logic of his own analysis now calls for.

DONALD WEINSTEIN
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ALBERTO CARACCILO. *L'albero dei Belloni: Una dinastia di mercanti del Settecento*. (Saggi, number 235.) Bologna: Il Mulino. 1982. Pp. 160. L. 12,000.

Alberto Caracciolo of the University of Rome, best known for his work on the free port of Ancona and coauthorship of a history of the Papal States in the early modern period, has embarked in this modest volume on a study of a dynasty of Italian merchant-bankers in the eighteenth century. At one time in the middle of the century, the Belloni family saw five brothers or cousins active in Bologna, Rome, Genoa, and Cadiz. Although their story bears some resemblance to the contemporary rise of the Rothschild family, the scale of their activity was never as large and their fortune, after a rapid rise, faded within the surprisingly brief space of two or three generations.

Indeed, the importance of this family, which never equalled that of the Renaissance merchant princes, rested chiefly on the founder of the dynasty, Gianangelo Belloni, and his nephew Girolamo. The family originated in Codogno in Lombardy, one of the most prosperous and highly developed agricultural areas in Italy and a small center of silk manufacture. From there, an already affluent Gianangelo made his way to Bologna where he made his fortune, as had so many before him, through business dealings with the papal government. He started as a tax farmer for tobacco and gin in the province of the Romagna and gradually acquired the more lucrative farm for Rome. Then he extended his banking connections to London, Paris, Lisbon, Cadiz, and the Indies, all the while pursuing a shrewd strategy for family advancement. In the absence of a direct male heir, Gianangelo's nephew Girolamo emerged as the principal bearer of the family for-

tune. Based in Rome, he became the greatest private banker in the papal capital. Girolamo not only advanced the family business but also became a trusted adviser in economic matters to Popes Clement XII and Benedict XIV. This activity involved him in a rather unsuccessful term as administrator of the Roman customs (1730-37). Girolamo crowned his career by acquiring the dignity of a marchese in 1750. His international reputation, however, rested on his economic writings, principally his *Dissertazione del commercio* (1750), a rather prosaic tract based on his Roman experience that won widespread European interest. He died in 1760 and was succeeded by his son Francesco, during whose lifetime the Belloni business declined, finally disappearing when the main bank closed in 1793.

The author is to be commended for the painstaking work of piecing together the details of the lives of the Belloni family, scouring records ranging from the notarial archives of Lodi through numerous Italian state archives and libraries to the chamber of commerce in Marseilles and the Royal Archives at Windsor Castle. Although Caracciolo found the relics of the family archive in disarray, over a period of twenty years (interrupted by other work) he was able to produce a respectable narrative. Its chief flaw, one of which Caracciolo himself is acutely aware, is to be found in the title that promises more than the book delivers. The title would suggest an intergenerational family history, but what we get is primarily a biographical study of Girolamo Belloni with some other family data thrown in. Indeed, this is what happened. Caracciolo started his work in the 1960s writing a traditional biography. Only when he returned to it in 1981 did he try to take account of the new trend in social history.

HANNS GROSS
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GIORGIO PETRACCHI. *La Russia rivoluzionaria nella politica italiana: Le relazioni italo-sovietiche, 1917-25*. Foreword by RENZO DE FELICE. (Biblioteca di Cultura Moderna, number 856.) Rome: Laterza. 1982. Pp. xxiii, 359. L. 24,000.

The opening of Italy's Foreign Ministry archives some years ago is now producing a rich harvest of authoritative studies of that nation's diplomacy. Giorgio Petracchi's book, based solidly on files in the Archivio Storico del Ministero degli Esteri, is an excellent sample of the bounty afforded by this source. The author has also consulted private papers, published material, and an exceptionally wide range of government records in the Archivio Centrale dello Stato.

One reason for this breadth of research is the

author's avowed intention to go beyond the mere political dimension. In practice, this precept is applied to the construction of Italian foreign policy rather than to the Italo-Russian relationship. Thus, Petracchi examines Italian business interests in Russia, Italy's public opinion (conventionally interpreted through press attitudes), and, inevitably, the ideological analogue of the Bolshevik revolution of 1917. These and more domestic factors are woven into a comprehensive analysis of decision making within Italy. But relations between Rome and Moscow are presented primarily in terms of formal diplomacy. The exchanges between Italy's socialists and Lenin, for instance, are dealt with only insofar as they helped to shape Italy's official position toward Soviet Russia.

Petracchi's narrative unfolds in three stages. First, the Bolshevik revolution itself reinforced the old Italian impression of Russia as an unreliable country to be treated with hostile reserve. Second, the parlous condition of the infant Soviet state tempted Italian governments to take up a British invitation to become involved in the Caucasus. Third, Rome's ultimate admission that the Soviets were here to stay necessitated a *modus vivendi* and *de jure* recognition of the new government. The book's leitmotiv, then, is the Italian "duplice politica." On the one hand, under the cloak of anti-Bolshevism and Italian economic imperialism, the policy of meddling in Russian internal affairs connoted the dismemberment of Russia. On the other, Italy's traditional pursuit of its aims by means of an equidistant stance in the international balance of power dictated acceptance of the new Russia, regardless of ideological coloring, as a weight in the post-World War I system.

The transition from the former to the latter position was effected by the Nitti ministry (1919–20), which liquidated the Caucasus venture. Petracchi puts this development into a broader historical perspective than either Enrico Serra (*Nitti e la Russia* [1975]) or Marta Petricoli (*L'occupazione italiana del Caucaso* [1972]). According to Petracchi, Nitti's readiness to normalize relations with Russia owed more to a concern to manipulate working-class politics in Italy than any expectation of cheap foodstuffs from the Ukraine. Yet even more important was Nitti's genuine anxiety about international tension, which he tried to ease by integrating the Soviets into the family of nations. It fell to Mussolini to complete Nitti's work. The detente of 1924 was facilitated unquestionably by the "antibourgeois" affinity between fascism and communism. Moreover, the Duce, always prone to go to extremes, came close to complementing recognition of Soviet Russia with an Italo-Russian treaty.

Renzo de Felice has written an informative preface to this work that, to some extent, steals the

book's thunder by providing a synopsis of themes and highlights.

ALAN CASSELS
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ANDREW L. ZAPANTIS. *Greek-Soviet Relations, 1917–1941*. (East European Monographs, number 111.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. 635. \$35.00.

Traditionally an area of primary strategic concern for tsarist Russia, the Balkans received less Russian attention in the interwar years, largely due to the new Soviet regime's limited military and economic capabilities. Nonetheless, the Soviet government and the Comintern sought to exert some influence in the region.

Relations between the Greeks and Soviets started off badly enough in the immediate post-World War I years. Prime Minister Eleftherios Venizelos, anticipating rewards of Turkish territory in return for continued loyal support of the Entente, agreed to provide three Greek divisions to augment French forces fighting the Bolsheviks in southern Russia. The Greek population in Russia, in 1917 estimated around 500,000, suffered with the retreat of Allied units during 1919. Moscow next perceived the Greek military presence in Asia Minor as an extension of British imperial interests and responded by providing Mustafa Kemal's growing nationalist forces with economic and military aid. In the aftermath of its defeat in 1922, Greece absorbed more than 1,300,000 refugees from Turkey. Because of this great burden imposed on the small, impoverished nation, Greek governments were unable to accept Greeks seeking repatriation from the Soviet Union.

Greece followed the lead of Britain and several other European states in establishing diplomatic relations with the Soviet Union during 1924 and remained the only Balkan state until 1934 to grant recognition. Intermittent negotiations for a trade agreement ensued, and the two governments ratified a Convention of Commerce and Navigation in 1930. The level of trade increased so noticeably that by 1932 the Soviet Union provided 10.09 percent of Greece's imported goods, occupying third place behind the United States and Britain. Although Greece's exports to the Soviet Union remained insignificant, Athens benefited from the low prices of Soviet goods and from the chartering of Greek merchant ships by Moscow. The remainder of the decade witnessed a decline from these high figures.

Greek governments demonstrated in varying degrees concern over Comintern policies and the activities of the Greek Communist party (KKE)

throughout the interwar period. The KKE experienced serious internal disputes until the Stalinist faction prevailed in 1928. The party's popularity suffered after it was pressured to adhere to the 1924 Comintern resolution calling for the establishment of an autonomous state of Macedonia and Thrace, a stand generally reinforcing Bulgarian nationalist policy. KKE voter support increased during the 1930s, but its members were early victims of John Metaxas's harsh rightist dictatorship.

Until the weeks immediately preceding the Nazi-Soviet Non-Aggression Pact, Moscow advocated the formation of an anti-Axis Balkan bloc. After the outbreak of war in September 1939, the Soviets showed a lack of interest in Greece, even after the Italian invasion in late October 1940. The author's last chapter, "Marita-Merkur-Barbarossa" (98 pages), provides in tedious, often repetitive detail the various interpretations surrounding the significance of the Greek stand against the Germans and the consequent delay in the Axis invasion of the Soviet Union.

Regrettably, the author has ineffectually presented his important subject. The reader is bombarded with countless undigested quotations, long explanatory footnotes, and one-sentence paragraphs. A judicious editorial hand would have cut the book's length in half and increased its interpretive analysis. Among the many critical gaps in analysis is the omission of discussion of the general foreign policy orientations of both Greece and the Soviet Union during the interwar years. The extensive research in diverse sources is, however, admirable.

S. VICTOR PAPACOSMA
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G. M. ALEXANDER. *The Prelude to the Truman Doctrine: British Policy in Greece, 1944-1947*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xii, 299. \$46.00.

In this book G. M. Alexander examines in detail the last phase of occupation and the immediate postwar period in Greece and the immense political and economic problems the country faced. These problems, the direct consequence of the upheaval Greek society experienced during the Axis occupation, were of such magnitude that eventually the country was engulfed in a bitterly fought civil war with repercussions lasting to this day.

Britain aimed at the establishment of a noncommunist parliamentary democracy in Greece. Such a regime functioning in conditions of long-term stability would best suit British interests in the eastern Mediterranean and the Middle East. The essential prerequisite for the success of British policy in

Greece was that these pressing political and economic problems be solved. As a result, British aid to the Greek government and intervention in Greek internal affairs during the war continued uninterrupted to the postwar period. It was thus decided that British troops should remain in Greece until the plebiscite on the future of the monarchy and elections were held; a military mission, a police mission, and an economic mission would provide the Greek government with the required technical advice. This decision, however, was contradictory to the policy Britain followed in Eastern Europe, which advocated the withdrawal of the Soviet Union's troops and the principle of national sovereignty. Following the liberation of Greece in October 1944 and the abortive KKE (Communist party of Greece) insurrection in December, the traditional political parties had the field to themselves. A number of governments were formed based on these parties, under watchful and frequently intervening British supervision. These administrations proved unable and sometimes unwilling to follow British advice on a number of crucial issues—such as the economy—let alone formulate and implement policies of their own on these questions. Politically motivated violence was another issue these successive governments failed miserably to control. Quite to the contrary, the Populist party, winner of the elections finally held in March 1946, from which the KKE and a number of republican parties abstained, introduced legislation that was flagrantly directed and ruthlessly implemented against the left and the communists. The Labour party, in power since July 1945, could not sanction much more drastic intervention, such as that proposed by Sir R. Leeper, British ambassador to Greece, although it did intervene decisively in a number of issues. By February 1947, however, London had decided to withdraw from Greece, by then engulfed in the civil war. The U.S. government agreed to take over.

This study focuses on British attempts to steer the highly polarized Greek politics toward a middle course in a situation rapidly leading to civil war. The British, however, share a large part of the blame for this polarization and the civil war that followed, a fact that this study does not mention. Their decisive intervention against the KKE and the EAM (National Liberation Front)—the latter embracing a much larger spectrum of opinion than the British cared to admit—and in favor of the politicians, whom they contemptuously referred to as the "old gang," set the pattern of polarization for the years to come. Although British policy is aptly set in its international context, the concomitant social repercussions in Greece are not examined. This study is the first approach to rely mainly on British state papers from this period. It succeeds by taking full advantage of this rich material to present British policy clearly,

thus contributing significantly to the understanding of this controversial and complex period.

PROCOPIUS PAPAISTRATIS
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JOHN D. TREADWAY. *The Falcon and the Eagle: Montenegro and Austria-Hungary, 1908–1914*. West Lafayette, Ind.: Purdue University Press. 1983. Pp. xix, 349. \$18.00.

John D. Treadway presents here an excellent, comprehensive analysis of Montenegrin–great power relations prior to World War I. With facility in the French, German, Russian, Bulgarian, and Serbo-Croatian languages, the author has examined all pertinent documentation and produced an authoritative work.

Treadway demonstrates that Montenegro was not in tow to Serbia and Russia. Prince (then King) Nicholas had strong ambitions to expand the Montenegrin kingdom and with an amazing *sang-froid* pursued them. His dream was a restored medieval Serb empire with himself as monarch. But when Serbia became independent and Austria occupied Bosnia in 1878, Nicholas simply looked to Turkish areas not yet absorbed, northern Albania and the Sanjak. Vienna consistently attempted to block this, since an expanded Montenegro would endanger the monarchy's control of its own Slav populations.

Montenegro's relationship with Russia was different; Russia had been its political and financial supporter for two centuries. Although relations between the two were generally smooth between 1879 and 1908, Russia never gave unquestioning support to Nicholas's strategems. When he backed Albanian Christians, the Malissori, in a revolt against the Turks, Russia and the other powers insisted that he maintain the status quo. Nicholas acquiesced but did not give up. Emboldened by the outcome of the Italo-Turkish War in 1911, he allied with Bulgaria and then with hitherto hostile Serbia to set the stage for the First Balkan War. Notwithstanding Austria's warning that it would find such action unacceptable, Serbian and Montenegrin troops occupied the Albanian coast and Nicholas entered Scutari in April 1913. He had flown in the face of the London Conference, where the powers had agreed to create an autonomous neutral Albanian state, and had tried to bargain with Berchtold, the Austrian foreign minister. The latter's threatened armed intervention and Nicholas's complete isolation forced him to evacuate Scutari. Montenegro fared somewhat better in the Second Balkan War, when it reluctantly supported Serbia and obtained additional adjustments to its Sanjak holdings.

Between 1913 and the outbreak of World War I,

Montenegrins were preoccupied with unification with Serbia, which, if not handled properly, could easily have threatened the general peace. Germany believed the unification inevitable, while Russia was much more vague on the matter. Austria threatened occupation of the entire Montenegrin coast if unification occurred. Italy would accept the Austrian move, but only if compensated with the Trentino, a price Vienna would not pay. The problem was overshadowed by the Sarajevo crisis, which exploded into war. Berchtold, to keep Montenegro from joining Serbia in the conflict, suddenly reversed Austrian policy and offered Nicholas extension into northern Albania and the Sanjak. But it was too late. The Montenegrin people now strongly identified with Serbia and allied with it against the Central Powers.

As Treadway points out in his conclusions, Montenegro's part in European affairs was disproportionate to its size and power, a result of its geographical location and the clashing policies of other states. Nicholas's actions affected each of these states, often making the Triple Entente uncomfortable while threatening to split the Triple Alliance because of Austro-Italian disagreement on the fate of the Adriatic. It is ironic that the Austrian empire and Montenegro, which for so long opposed each other, were both destined to disappear in 1918.

GERARD E. SILBERSTEIN
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ALEX N. DRAGNICH. *The First Yugoslavia: Search for a Viable Political System*. (Hoover Press Publication, number 284.) Stanford: Hoover Institution Press. 1983. Pp. 182. \$24.95.

This brief survey of the political history of interwar Yugoslavia focuses on the problems of creating a workable political system in a state that was established hastily in the aftermath of World War I by peoples with different political traditions and expectations. It was a multinational state founded by three Southern Slav nationalities, the Serbs, the Croats, and the Slovenes, and also included Southern Slavic groups recognized after World War II as separate nationalities, the Macedonians and Yugoslav Muslims, as well as non-Yugoslav national minorities. Yugoslavia was founded in the euphoric postwar period as a democratic state with a relatively liberal constitution, a unicameral parliament (the *Skupština*), and a Serbian king. It became a royal dictatorship in 1929.

Alex N. Dragnich describes in detail the rise and fall of cabinets, shifting party coalitions, and the major political parties and their leaders. He portrays King Alexander as a "sincere Yugoslav" (p. 150) who worked hard to keep the state together. He

argues that the Yugoslav state was founded prematurely, before certain basic issues had been resolved, and shows how the Serbs and the Croats misunderstood each other from the very beginning. The Serbs assumed, perhaps naively, that the agreements made by the various Yugoslav representatives before the state was formed reflected a general consensus and that the unitary constitution of pre-war Serbia could be transferred to the Yugoslav state. The Slovene and Yugoslav Muslims generally supported the unitary concept. The Croats, however, had fought hard to preserve their "states rights" and national identity in the Austro-Hungarian empire and were apprehensive about their place as a minority in the new Yugoslavia. From the abstention of the Croatian Peasant party delegates to the Constituent Assembly in 1921 through periods of passive resistance, political participation, bloodshed, and suppression, the Croatian Peasant party leaders threatened secession, demanded federation, and finally won a semiautonomous status for Croatia with the *Sporazum* (Agreement) on the eve of World War II. The Croats had one major party and one consistent policy. The Serbs had neither. The conflict over the very nature of the Yugoslav polity undermined political consensus under the relatively liberal Vidovdan constitution, kept the Yugoslavs divided under the dictatorship, and finally began to reach a solution in the last months of the state. Foreign policy is discussed only when it touches on political issues, and economic problems are relegated, almost as an afterthought, to a ten-page chapter in which Dragulich attempts to demolish the claims made by Rudolf Bičanić in a polemical work in 1938 that the Croats were exploited economically by the Serbs.

The author bases this work primarily on Yugoslav sources. The footnotes are sparse and in many places inadequate. Dragulich claims (p. 4) that the Croats lacked "their own political institutions" and a national parliament before 1914. What then was the Croatian *Sabor*? Although he attempts throughout to be objective, he does show a consistent anti-Croatian and anticommunist bias. In the conclusion, Dragulich argues that the Serbs are charged unfairly with seeking political and economic hegemony in Yugoslavia. He bases his disclaimer of economic hegemony on the quite inadequate chapter on economics and argues that the Serbs were forced to play a major political role because of Croatian absenteeism at first and "by their penchant for engaging in obstructionist tactics" later (p. 151).

This is a useful book. It may not be the definitive work on the subject, but it makes coherent a large mass of complex and highly charged material, and should spark more work on interwar Yugoslavia.

ELINOR M. DESPALATOVIĆ
Connecticut College

WAYNE S. VUCINICH, editor. *At the Brink of War and Peace: The Tito-Stalin Split in a Historic Perspective*. (East European Monographs, number 124; Brooklyn College Studies on Society in Change, number 19; War and Society in East Central Europe, number 10.) Brooklyn: Social Science Monographs or Brooklyn College Press; distributed by Columbia University Press, New York. 1982. Pp. xi, 341. \$27.50.

The famous Cominform resolution of June 28, 1948, was a landmark in the history of Yugoslavia, of the USSR, of international communism, and of East-West relations. The basic decisions had already been taken before the edict of excommunication was issued: by Stalin, to compel Yugoslavia's submission to his will; and by Tito, to resist. How and why those decisions were taken remains a fascinating subject for historical research and is central to our understanding of Soviet policy and of Soviet-Yugoslav relations. The basic material on events leading up to the break, the exchange of letters between the Soviet and Yugoslav leaders, was published by the latter at the time in order to make their case to the world. Since then we have had some revealing personal accounts from the Yugoslav side (by Tito, Kardelj, Tempo, Djilas, Dedijer, and others) but virtually nothing from the Soviet side. As Lucien Karchmar shows in his excellent contribution to this volume, Soviet historians have pointedly ignored the subject or have dealt with it in conformity with the official political and ideological line at the time of writing.

Wayne S. Vucinich, in organizing a conference on the thirtieth anniversary of the Tito-Stalin break and in editing the book that emerged from the conference, wisely refrained from concentrating on the events of 1948, on which there is little new to say, and instead turned his authors loose on a wide range of topics that might be called consequences of the break. Hence, we have two brief but perceptive essays by William Zimmerman and Phyllis Auty on Yugoslav foreign policy since 1948; two competent and data-laden studies of the Yugoslav economy, the first on the early years (by Jozo Tomasevich), the second on the succeeding decades (by George Macesich); and two pieces on the theory and practice of socialist self-management, Yugoslav-style, which address but do not answer the key question whether the new post-1948 political and economic institutions were a product of the break or would have developed anyway. There is also a study of Yugoslav elites by Leonard Cohen having little to do with 1948 or relations with the USSR; an account by Ivo Banac of "Cominformist" activity against Yugoslavia and of Tito's remarkable success in coping with it; and a detailed description by Béla Kiraly (included perhaps to give substance to the title and subtitle of

the book) of Soviet military planning in 1948–51 for an attack on Yugoslavia—an attack that never took place although the planning served the purpose of psychological warfare. On one of the most significant topics, the impact of the break on international communism, Andrzej Korbonski has some interesting things to say about the decline of the influence of Titoism in the 1970s but pleads lack of space in not developing his thoughts further.

The real find in this book, in this reviewer's opinion, is a searching inquiry by Nicholas Pappas into Yugoslavia's connection with the Greek civil war of 1945–49, especially what happened after June 1948. It is an incredibly involved story of the classic Macedonian question at its best, or worst: nationalism and communism intertwined, great power politics and Balkan intrigue, plus factionalism and personal animosities. Pappas has not unraveled it entirely, but, by using all the available Greek and Yugoslav sources, he has gone a long way toward doing so.

JOHN C. CAMPBELL
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LEE CONGDON. *The Young Lukács*. Chapel Hill: University of North Carolina Press. 1983. Pp. xii, 235. \$21.00.

The puzzle that the evolution of the thinking of the young György Lukács has presented to his critics and admirers alike has finally been explored in this pioneering work. The brooding, often cold hearted and calculating young man depicted by Lee Congdon certainly possessed an inordinate amount of self-confidence, which enabled him to follow his own path of development. He progressed from crisis to crisis, defying conventions and traditions in his personal relationships and his attitude toward society at large. The image emerging from the volume is that of a series of still photographs of bygone days one often finds in faded family albums. As such pictures often do, they reveal a panorama of dynamic background that says more about the man than his face does. Yet, I must bemoan the fact that this volume stands alone in exploring that fascinating milieu that gave us Oszkár Jászi, Mihály Károlyi, Ervin Szabó, and others, men who have done so much in leaving their imprint on twentieth-century history. This observation does not detract from the value of this most interesting work.

At each turning point of Lukács's development, he encountered a *femme fatale* who helped him to the next stage of his thinking. His meeting with Irma Seidler compelled him to explore the meaning of modern drama and the essay. His finding that "the good man was a holy sinner whose purity was of another, higher order" (pp. 68–69) foreshadowed

Lukács's later justification of violence and revolutionary terror. His marriage to Ljena Grabenko led him to believe in the utopian notion that "heaven, understood as the reign of perfect justice, could be established on earth" (p. 143). From that time on, largely under the influence of his radical wife, Lukács believed that Russia was to deliver salvation to the world and establish a utopia for which so many had given their lives throughout history. Finally, Lukács's meeting with Gertrud Bortstieber helped him to overcome what he believed to be an "insoluble alienation" between men and women, a notion that exercised Lukács's mind from his early childhood.

After the outbreak of the Russian Revolution in 1917 and his experiences in the Hungarian Soviet Republic in 1919, Lukács arrived at an interpretation of Marxism that was pure dogmatics and, as such, was "invulnerable to external criticism" (p. 186). By that time, he provided ideological support for unrestricted despotism.

Congdon's work is more than just first-rate biography. It is not only the first such work on Lukács in English, but it is also an excellent exposé of the atmosphere in which Lukács's thinking took place. It is written clearly and well. It is, however, an unfinished work; one cannot help but be impatient in expecting its continuation in the near future. Nevertheless, it does provide the reader with a better understanding of a thinker who was caught up in the political turmoil of his time and who was unable and unwilling to regard his fellow human beings with the compassion they richly deserve.

A useful bibliography and an excellent index are attached to this work.

JOSEPH HELD
Rutgers University

JOSEF HARNA. *et al. Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1929–1939* [Materials for the Political, Economic, and Social History of Czechoslovakia, 1929–39]. (Sborník k Dějinám 19. a 20. Století, number 8.) Prague: Ústav Československých a Světových Dějin ČSAV, Oddělení Novověkých Československých Dějin. 1982. Pp. 354.

This study is a compilation of "materials for the political, economic, and social history" of the Czech provinces of Czechoslovakia from the outbreak of the depression in 1929 to the German invasion of March 1939, assembled by a Czech authors' collective. It is the eighth volume in a series of preliminary findings and conclusions that provide a basis for the multivolume *Přehled dějin Československa* (Survey of the History of Czechoslovakia) now being prepared by a group of Czech and Slovak historians. The

preceding volume, which covered the period from the creation of Czechoslovakia in 1918 to 1929, was reviewed in the June 1983 issue of the *AHR* (p. 712).

The authors of the collective who prepared the book under review rely entirely on earlier Marxist studies, and in their interpretations of the events of the turbulent decade 1929–39 they adhere closely to the orthodox communist line. The First Czechoslovak Republic is denounced as a “bourgeois” state whose leaders served class interests and the imperialist West. Supported by figures and tables that give the statistics for the rise in unemployment, strikes, lockouts, and clashes with police, the authors show how the depression brought about severe social tensions. More importantly, the depression politicized the already disgruntled national minorities: the Sudeten Germans, the Hungarians, and the Poles. This was especially true of the Sudeten Germans who were hit hardest by the economic dislocations of the times since their industries happened to be most dependent on world markets. Under the leadership of Konrad Henlein and his Sudeten Nazi party, the Sudeten Germans demanded full political autonomy within the Czechoslovak Republic. After Hitler came to power in Germany on January 30, 1933, they became an outright irredentist element on the extreme right of the political spectrum.

In dealing with the power struggles in Czechoslovakia during that time, the authors cover the full array of political parties, effectively depicting the shifting alliances that were made among the Czech and German socialist as well as nationalist parties of the center. The main focus, however, is on the Czechoslovak Communist party, the fourth largest party in Czechoslovakia, led by Klement Gottwald. It is identified as the party of the proletariat and assigned the premier role in an early and energetic resistance to native and foreign fascist forces as the Czechs found themselves more and more on the defensive. Following a directive from the Comintern, the communists tried to form a popular front against fascism with some of the “reform” factions from the bourgeois parties.

The Munich crisis is presented as a result of the bankrupt policy of reliance on the Western allies. President Eduard Beneš is faulted for succumbing to French and English pressure and making concessions on the eve of the Munich agreement, which was concluded on September 29, 1938. The authors argue that Beneš should have worked more closely with the Soviet Union, the only country to condemn officially the appeasement at Munich. The Second Czechoslovak Republic, formed after the cession of the Sudetenland and the abdication of Beneš, is shown as a very repressive, quasi-fascist regime that outlawed the Communist party. The communists turned to illegal means of opposition, which they continued after the German occupation of Czecho-

slovakia on March 15, 1939. The volume ends before the Hitler-Stalin Pact of August 1939.

Within the constraints of this Marxist analysis, which makes exaggerated claims for the communists as the defenders of democracy in the last years of Czechoslovak independence, the book under consideration contains valuable information and presents the reader with a comprehensive overview of this tragic decade in the history of Czechoslovakia.

BARBARA REINFELD

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HARRY E. DEMBKOWSKI. *The Union of Lublin: Polish Federalism in the Golden Age*. (East European Monographs, number 116.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. vii, 380. \$27.50.

On July 1, 1659, the enactments of the Diet at Lublin confirmed the real union of the Kingdom of Poland and the Grand Duchy of Lithuania in “one inseparable and indistinguishable body.” On July 4 this new constitutional arrangement, which created one dual confederative state called the “Commonwealth of Both Nations,” received the royal assent of King Zygmunt August (1548–72). After long and protracted negotiations between Poland and Lithuania, there thus emerged in Eastern Europe one state comprising two political units. United on the basis of equality, these units were ruled by a jointly elected monarch (who was both the king of Poland and the grand duke of Lithuania), participated in a joint diet, coined a common currency, and pursued a common foreign policy. Each unit, however, retained its separate state offices, army, treasury, judiciary, and administration.

Although the balance of power, which was supposed to be based on the principle of equality between Poland and Lithuania, quickly tipped in favor of the former, the Union of Lublin remained largely undamaged. In 1658, in the 1760s, and finally in 1791 attempts were made to restructure the union. For various reasons, however, these endeavors from within came to naught; the union managed to survive until it was destroyed from without, through the third partition of the commonwealth by Austria, Prussia and Russia in 1795.

Harry E. Dembkowski's PhD research, which gained recognition by the Kościuszko Foundation doctoral dissertation award in 1976, forms the subject of this monograph. The author states that its theme and purpose is to reveal “how and why the edifice of the Polish-Lithuanian Commonwealth was erected at Lublin.” He does not intend to write a definitive study about all factors that led to the events of 1659. He chose instead a more modest

approach: only "to examine the political-legal relationship between Poland and Lithuania that culminated in the union." He was motivated to write the book because "no serious study of this subject has ever ventured into print in the English language" (pp. v, 1).

The monograph comprises twelve chapters. The first three discuss the geopolitical units and populations that eventually became part of the commonwealth; the nature of the Polish-Lithuanian dynastic union, or the Jagiellonian heritage, after 1386; and the major political movement in Poland, known as the "execution of laws," relating primarily to territorial centralization and the union with Lithuania. Chapters 4 to 9 deal primarily with proceedings of the Diets from 1548 to 1569, since at their sessions matters concerning the union were discussed in detail. The last three chapters examine the significance and repercussions of the union on the post-1569 history of the commonwealth, analyze the union in terms of the concept of federalism, and compare it with other realized or abortive unions. These are followed by a bibliographical essay, appendixes, notes, a bibliography, and an index.

This work has many attractive attributes, among which organization, clarity, and readability must be singled out, but it also contains certain weaknesses. The most obvious is the author's failure to bring the bibliography up to date. Among primary sources, one will look in vain for Irena Kaniewska's *Diariusz sejmiku lubelskiego 1566 roku* (Wrocław, 1980) or, among secondary materials, for such an excellent monograph as *Sejm Walny Koronny w latach 1506–1540* (Warsaw, 1980) by Waław Uruszczak. The second problem concerns manuscript sources. Even though the author deserves praise for his efforts to find manuscripts pertinent to his project in several Polish archives and libraries, he still must be criticized for not familiarizing himself with all holdings of the journals of proceedings of the Diets. These, after all, are indispensable for a thorough analysis of the subject. A glance at the lists of Henryk Olaszewski, in "Nowe materiały do *Chronologii sejmów polskich*" (*Czasopismo Prawno-Historyczne*, 9 [1957]: 242) will reveal what the author has failed to consult. The third deficiency is the author's unfamiliarity with Lithuanian historiography. An uninformed reader might be left with the erroneous impression that Lithuanian publications on the Union of Lublin could be considered as a *quantité négligeable*. One can sympathize with Dembkowski that the Lithuanian language is not easy to master. He does not, however, list many Lithuanian contributions in West European languages, including Jonas Žmuidzinaitis's *Commonwealth polono-lithuanien ou l'Union de Lublin (1569)* (Paris, 1978).

Other criticisms are of a minor nature. The author fails to provide a short description and

evaluation of primary sources, manuscript and printed. His discussion of federalism and unions is quite sketchy, and he lacks emphasis on the event in 1569 as a union of nobiliary states. Moreover, his choice of secondary literature to explain such events as the Union of Hadiach (1658) is often not the best. He has provided two rather poor maps and has left the reader without full English translations of important documents relating to the enactments of 1569. Lastly, there can be no excuse for not adopting the now well-known Library of Congress system of transliteration of the Cyrillic alphabet or for not spending some extra time on proofreading. With regard to the latter, the text has too many typographical errors and in many instances proper diacritical marks are missing from Polish words.

When all is said, the book's virtues outweigh its flaws. It is important to note that through this scholarly monograph Dembkowski has made a significant contribution in English to the study of Polish and Lithuanian history in particular and East European history in general. He may have also provided that long-awaited impetus that should result eventually in the publication of a comprehensive history of the Jagiellonian union in the English language.

A. B. PERNAL
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ANDRZEJ WALICKI. *Philosophy and Romantic Nationalism: The Case of Poland*. New York: Clarendon Press of Oxford University Press. 1982. Pp. 415. \$39.00.

Andrzej Walicki is best known for at least two books in English: one on Russian Slavophile ideology; the other a history of Russian thought from the Enlightenment to Marxism. According to Isaiah Berlin, they are "the best and most complete works in this field published in this century." In Poland Walicki is recognized not only as a Russian expert but also as a leading historian of Polish political, social, and religious thought. This book, specially written for the Western reader, draws on his Polish writings.

According to Walicki, Polish history is little known in the West and Polish intellectual history is virtually unknown. This is to be regretted, he says, because European history cannot be understood if East Central Europe, of which Poland is an important part, is ignored. Poles have made a significant contribution to such European movements as Romanticism, for instance, or Hegelianism. Russian intellectual history also cannot be understood properly without taking into account Polish-Russian interaction. (Consider, for example, the influence of Polish thinkers on the formation of Russian populism.) The author is especially angered by those

Western scholars who fail to acknowledge the Polish role in nineteenth-century European revolutionary movements. Contemporaries were well aware of the Polish contribution. Walicki cites books on 1848 where Poland is mentioned only in passing, and works on nationalism that, in his view, profoundly misinterpret the Polish phenomenon. He strongly objects to scholars who classify Polish nationalism as ethnic, "secondary," or aristocratic, and he considers Hans Kohn's famous dichotomy of "Western" and "Eastern" nationalisms to be fundamentally wrong. (Kohn described Western nationalism as political, rational, enlightened, tolerant, and open, and Eastern as irrational, ethnic, intolerant, and so on.) Nineteenth-century Polish nationalism, Walicki points out, was political, not linguistic or ethnic, just as the prepartition Polish nation had been a multilingual, polyethnic community based on a shared political history and common political institutions.

This fact has to serve as a point of departure for the study of Polish thought in the nineteenth century. How does a political nation react to the liquidation of its state? In a sense, it ceases to exist once the political form is dissolved. Indeed, there were Poles who drew this conclusion. But most Poles refused to concede that with its partitions Poland had left the stage of history. They rethought the consequences of a loss of statehood and developed programs for the restoration of independence. First, they reflected on the relation between state and nation and concluded that a nation as a community held together by political ideals and political efforts survives the loss of a state. Second, they reformulated the concept of the nation from one consisting of nobility alone (which had been the case in old Poland) to a nation of all classes. The peasant question understandably was a central issue here: some revolutionary nobles proposed to abolish their own class in order to make peasants into Poles. Third, much thought was devoted to the question of Poland's place in the family of nations and to the question of how to reconcile nationalism with an allegiance to universal human values, especially religion. Finally, the Poles sought to elucidate the relation of ethics to politics, of power to principle. The essence of the "political Romanticism" of the Poles, says Walicki, was expressed in the idea of morality in politics.

These broad themes, formulated in the first part of the book, are developed in part 2, which bears the title "In Search of a 'National Philosophy,'" and in part 3, "National and Religious Messianism." The former presents the ideas of Hoene-Wronski, Cieszkowski, Trentowski, Libelt, Kamienski, and Dembowski, and closes with a chapter on the "Conservative Critics of Philosophy," of whom the most remarkable was Rzewuski. The messianists discussed are Mickiewicz, Slowacki, Krasinski, and, once again, Cieszkowski—while Norwid is presented

at the end as the leading critic of messianism. Walicki views national philosophy and romantic messianism as related but distinct "approaches to the same problems of religious and national regeneration." (His definition of Polish messianism is clarified on pages 240–42, and on pages 91–96 he presents a concise summary of what he means by "national philosophy.") Walicki treats his subjects as thinkers with their own concerns, approaches, and solutions. He does not reduce them to expressions or illustrations of an "idea" or principle. He offers precise and fruitful conceptual distinctions, subtle arguments, suggestive comparisons, and erudite commentaries. There is also a "personal touch." Hoene-Wronski, we learn, persuaded a banker from Nice, one P. J. Arson, to give away his entire fortune for a promise "to disclose the mystery of the Absolute." (In our time, one imagines, Hoene-Wronski would be with Rand or the Hudson Institute.) As for Kamienski, "when he was crossing the border of the Russian Empire he experienced the 'strange feeling of personal freedom.' No wonder that he decided to settle in Switzerland permanently."

The concluding part consists of two independent essays. One sketches out the transition in Polish thought from Romantic (and political) nationalism to the ethnic and xenophobic integral nationalism of the late nineteenth and early twentieth centuries. The other deals very thoroughly and carefully with the subject of Marx and Engels and Poland. When the author, however, explains why the "classics" supported Polish nationalism but opposed the Czech national aspirations, despite the higher economic development of Bohemia, and argues that this proves that they were not thinking in purely economic terms, he overlooks their own explicit explanation. The "Revolution and Counter-Revolution in Germany" clearly states that Bohemia's economic and cultural development was due to the Germans, not the Czechs.

Walicki's book is a major scholarly accomplishment that does for Poland what his other books have done for Russian studies. With it he proves that Poland is much too important a subject to be left to Polish experts alone. The specter that really haunted nineteenth-century Europe was Polonism. Lord Acton put it very well more than a hundred years ago: the partition of Poland, he wrote in his "Nationality," "awakened the theory of nationality in Europe, converting a dormant right into an aspiration, and a sentiment into a political claim. . . . Thenceforward there was a nation demanding to be united in a State—a soul, as it were, wandering in search of a body in which to begin life over again; and, for the first time, a cry was heard that the arrangement of States was unjust—that their limits were unnatural, and that a whole people was de-

prived of its right to constitute an independent community."

ROMAN SZPORLUK
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WAYNE S. VUCINICH, editor. *The First Serbian Uprising, 1804–1813*. (East European Monographs, number 107; Brooklyn College Studies in Society in Change, number 17; War and Society in East Central Europe, number 8.) Boulder, Colo.: Social Science Monographs or Brooklyn College Press, Brooklyn; distributed by Columbia University Press, New York. 1982. Pp. xi, 389. \$27.50.

The First Serbian Uprising, 1804–13, followed almost at once by the second in 1813–15, resulted in Serbia's autonomy and started a roughly 100-year-long process that ended with the disappearance of the Ottoman presence in Europe and the establishment of the modern Southeast European states. This first uprising, therefore, signals the beginning of a new phase of the Eastern Question closely interwoven with great power politics in the nineteenth century that, finally, culminated in the First World War. Important as this event was, the present volume is the first attempt in English to present it in full and in detail.

The volume has two distinct parts. The first of these, chapters 1–3, 6, 9, and 12, describes the history of the uprising, including the military operations and the problems faced in the Serbian camp. The second part (chaps. 4, 5, 7, and 8) deals with foreign relations and the attitudes of the great powers and with special topics such as military organization and the role of the Orthodox church. The final chapter summarizes the impact of the uprising on the various people of the Balkans. It would have helped the reader if the volume had grouped these distinct parts together instead of following a roughly chronological progression irrespective of topic.

Roughly one-third of the volume was written by the editor, Wayne S. Vucinich, which minimizes, to some extent, the problem inherent in multi-author volumes: the diversity of styles and repetition. Nevertheless, one misses continuity of narration and, especially, of treatment when a given topic is handled by two authors. This is the case in the chapters written by Stanford J. Shaw and Enver Ziya Karal on Ottoman policy and in those discussing Russian attitudes contributed by Roger V. Paxton and the editor.

Vladimir Stojančević's chapter, "Karadjordje and Serbia in His Time," is an uncritical paean to the leader of the uprising and should have been omitted. The work of the other contributors is solid, reliable, if slightly too factual and somewhat lacking

in analysis. The most thought-provoking chapters were contributed by Emanuel Turczynski on Austro-Serbian relations and by Michael B. Petrovich on the role of the Orthodox church.

In spite of these criticisms, the editor and the authors achieved their aim. What we get from them is a broadly conceived and well-executed history of the First Serbian Uprising to which all those interested in Serbian, Balkan, and diplomatic history can turn with confidence to find all the information they might need. The book belongs not only on the shelves of every library but also on those of scholars interested in these topics. Easily readable print and good footnoting, including rich bibliographic information, add to the volume's value, but the absence of an index is regrettable.

PETER F. SUGAR
University of Washington

L. V. CHEREPNIN. *Voprosy metodologii istoricheskogo issledovaniia: Teoreticheskie problemy istorii feodalizma* [Questions of the Methodology of Historical Investigation: Theoretical Problems in the History of Feudalism]. Moscow: Nauka. 1981. Pp. 278. 2 r. 70k.

Academician L. V. Cherepnin (1905–77) was perhaps the most celebrated specialist on the middle centuries of that millennium (from the ninth to nineteenth centuries) labeled "feudal" in Soviet historiography. The editors of this first volume (issued by the commission named to publish Cherepnin's scholarly legacy) stress the continuing topicality of his work and its relative inaccessibility to scholars. They include articles, speeches, and commentaries composed and delivered from 1950 to 1975. They put aside the even less accessible work of the previous two decades.

The materials are grouped by theme: (1) the methodological significance of Lenin's works for the study of feudalism, (2) periodization of Russia's feudal history, and (3) Russian absolutism. The collection is introduced by V. T. Pashuto, Corresponding Member of the Academy of Sciences and Cherepnin's close collaborator, whose key professional focuses also find expression here. All but three of the twenty items have appeared in prominent journals and in various collections published following major discussions in the profession during the 1960s and 1970s. Printed for the first time are two articles (parts 2 and 17) and one speech (part 16)—a total of about fifty pages. Four entries are not Cherepnin's alone; three major items were coauthored with Pashuto (parts 4, 5, and 11), and several others explicitly depend on this collaboration.

The pivotal theme of the book, as stated by Pashuto, is the application of the theoretical wealth of Lenin's historicism to the study of feudalism

(p. 5). In addition to methodological and theoretical contributions, Cherepnin is commended for his emphasis on party-spiritedness (p. 7) and for the assertion that "the burning task of historical science" is "not to 'rethink' Marxism-Leninism in the light of new data but, on the contrary, to assimilate the new data of social life in the light of Marxism-Leninism" (p. 11). Since Cherepnin was already at an advanced stage of his career when he came to his first theoretical essay (1949) and to party membership (1957), he lacked the assurance and vigor of argument that comes of long experience with theoretical and methodological disputation. He tended to favor the historiographical and textological approaches of his earlier training. The first two articles on Lenin, for example, exhibit in part a "switching over" to a new primary source. Lenin's every word on feudalism is catalogued—without, unfortunately, the critical and imaginative attention to context that informed Cherepnin's masterpiece, part 1 of *Russian Feudal Archives* (1948). Cherepnin's reluctance to depart from the text produced an extremely conservative edition of Marxism-Leninism. In brief, his methodology was at the same time his theory. For his conception of feudalism, he concentrated less on Marx than on Lenin and, thus, reflected the attempt in recent decades to provide a Russian Marxism to nourish Soviet historians. Evident in Cherepnin's independent work is his preference for historiographical surveys, the statement of research priorities, and, above all, the reconciliation of sparring colleagues.

This collection exhibits the requisite party-spiritedness that holds the Soviet historian accountable not only to his sources but also to the changing party guidelines on how to use them. The same may be said of the selection, ordering, and editing of the contents. The editors might have added another half-dozen articles consonant with the book's general title and three themes. If some might have seemed repetitious and others even more historiographical in orientation than those included, one at least was clearly theoretical-methodological: "J. V. Stalin on Russian Feudalism" (*Uchenye zapiski*, 156 [1952]). New requirements dictated not only this omission but also the expunging of lines, paragraphs, and pages from articles connected to Stalin's theoretical-methodological contributions, especially passages dealing with the relative independence of the political superstructure from the economic base (parts 5–7), with attacks on subsequently rehabilitated historians (for example, p. 111), and other discredited interpretations (for example, p. 110). At times ellipses are not indicated (pp. 78, 174); at times they obliterate lines stating the article's purpose (pp. 104, 117); at times they omit quotations to which Cherepnin's comments are addressed (see, for instance, parts 13 and 15). Unsignaled changes

are made from the original (for example, pp. 76, 147). Far from facilitating access to Cherepnin's published work, this collection cannot be used without reference to the original publications, to the full text of key debates, and to the history of Soviet historiography in the postwar period.

The editors' choice of an organizational principle that eschews chronological development even within themes, as well as their failure to provide a historical context within which to place Cherepnin's thought tends to impose on these materials an illusory consistency, to make of them a monument far removed from the fierce and complex historical and ideological battles of recent decades. What emerges is how Cherepnin's colleagues and editors would have us see him. By deciphering the book's messages, we appreciate the genuine topicality of this presentation of Cherepnin's legacy for current debates at the Institute of History of the USSR.

JOAN AFFECTICA
Smith College

B. A. RYBAKOV. *Kievskaja Rus' i russkie kniazhestva, XII–XIII vv.* [Kievan Rus and the Russian Principalities, Twelfth to Thirteenth Centuries]. Moscow. Nauka. 1982. Pp. 588. 4 r. 90 k.

JOHN FENNEL. *The Crisis of Medieval Russia, 1200–1304.* (Longman History of Russia.) New York: Longman. 1983. Pp. xiii, 206. \$14.95.

Surveys of the early history of the Eastern Slavic peoples are sorely in need of updating in order to incorporate new research and interpretation—especially for historians seeking to enrich their appreciation of the antecedents of more modern issues in Russian and Soviet history without getting bogged down in the technical aspects of medieval historical inquiry. John Fennell and B. A. Rybakov have sought to meet this need in these two works. Both are eminent medievalists who have contributed numerous specialized monographs in their fields and who are accustomed to making the difficult transition from monograph to survey. It is especially gratifying to witness two recent attempts to interpret Russia's remoter past that reflect the erudition and experience of two of the most distinguished scholars in the field.

Although both Fennell and Rybakov offer revised overviews of the history of the early Slavs, it is clear to this reviewer that Fennell's *Crisis of Medieval Russia* better meets the needs of the reader than does Rybakov's *Kievskaja Rus'*. Fennell provides an especially readable study of one of the most chaotic periods of Russian history—the thirteenth century. Any student of medieval and modern Russian history would profit from this concise and provocative

overview of this troubled era. Fennell offers a compelling analysis of the causes of conflict among the princes of thirteenth-century Russia and of their impact on the lives of the inhabitants of northern and southern Russia. While inviting the reader to investigate with him the minutiae of princely politics described in the various chronicles of the period, he never entraps one in the mire of detail. Indeed, no historian has heretofore been able to create a coherent picture of this chaotic era without superimposing a model of political behavior not found in the sources. Fennell is true to his source data and brings it strikingly to life. One gains a vivid picture of why princely power declined after Prince Vsevolod III (1176–1212) and remained at its nadir until the middle of the fourteenth century.

Fennell is especially engaging in his analysis of the rule of Prince Alexander Nevskii (1241–53), usually thought to be the heroic grand prince who spared his principedom the onslaught of Swedes and Teutons and imposed order on a chaotic world. Fennell's Nevskii is considerably less heroic: his victories over Western enemies appear insignificant; far more significant appear his sycophantic behavior toward the Tatars and the failure to plan for succession to his throne. To Nevskii can be credited, to believe Fennell, a large measure of the chaos of thirteenth-century Russia.

Likewise intriguing and refreshing is Fennell's evaluation of the impact of the Tatar invasions on the history of Russia. He challenges the conventional wisdom that suggests total devastation and the cessation of cultural development and contact with the outside world. He specifically challenges B. A. Rybakov's assertion that Russian lands were "bled dry" at the hands of the Tatars (p. 89). Fennell argues convincingly that physical destruction was minimal, that cultural activity continued, and that interaction with the outside world was by no means cut off.

No overview of a period as chaotic as the thirteenth century can be expected to cover every aspect of society. Nevertheless, it would have been helpful to the reader and supportive of his thesis had Fennell chosen to include more literary and artistic evidence and to compare Russia with contemporary medieval society in the West. Be that as it may, the clear rendering of the interrelationships among the squabbling principalities of thirteenth-century Russia is a veritable tour de force and provides a major contribution to the field.

Whereas Fennell has concentrated on a single century and has utilized a single set of sources (the chronicles), B. A. Rybakov covers the history of the Slavs from the sixth century B.C. through the thirteenth century A.D. and has utilized every conceivable sort of evidence from the archaeological, with which he is so familiar, to the literary and

artistic. His book is macroscopic and, in this reviewer's opinion, he attempts to cover too much material without providing real coherence. Rybakov aspires to provide the reader with an overview of pre-Kievan, Kievan, and post-Kievan Rus through meticulous analysis of the sources. He addresses political, social, economic, ethnographic, and cultural issues in his effort to define the historical dynamic of the Kievan period. In attempting to cover so much, he has accomplished very little. Aside from some wonderful new maps, the reader has little to go on, save the conventional wisdom that Kiev has a long prehistory and a catastrophic posthistory due to feudal disintegration and Tatar devastation. Other than the oft-repeated reminders that Slavic history is long and rich and that Kiev marks an especially rich phase in the feudal development of Russian history, the reader has little framework for charting a course through the apparently arbitrary selection of material from Rybakov's earlier monographs. Specialists would be well advised to return to these works rather than to this six-hundred-page potpourri, which helps little in placing Kiev in historical perspective. Let us hope that Rybakov himself or another Soviet specialist on Kievan Rus will pen a more succinct rendering of Kievan history based on recent scholarship, including Rybakov's work on handicrafts, chronicles, and folk literature, both in the USSR and in the West.

ELLEN STISKIN HURWITZ
Lafayette College

I. A. EMEL'IANOVA. "Vseobshchaia istoriia prava" v russkom dorevoliutsionnom pravovedenii, XIX vek ["A General History of the Law" in Russian Prerevolutionary Jurisprudence in the Nineteenth Century]. Volume 1. Kazan: Kazan University Press. 1981. Pp. 157. 1 r. 36 k.

This is the first of two works by I. A. Emel'ianova to be devoted to the question of the origin of the general history of law in Russia. A projected second volume will deal with the postreform period between the late 1860s and the early 1880s. The goal is to trace the origin of the general theory of state and law to its nineteenth-century university beginnings. Was there at that time an independent science of the general history of state and law? Very little attention has been paid to this question in Soviet judicial literature. There has been no fully bibliographical or historiographical study of this question either in Soviet or prerevolutionary literature.

This book is divided into three principal sections. The first two deal with the question of legal thought in Russian education from the beginning of the nineteenth century to the end of the prereform period. The third section is devoted to the contribu-

tions of individual Russian scholars to the development of a general history of state and law.

There were many courses of study and books on the general theory of state and law during the nineteenth century. In Russian universities this study was considered an integral part of both general and national political history. The great attention paid to the history of state and law was apparent by the beginning of the sixties. A faster development in the form of courses, books, and archives began at that time and continued to the beginning of the eighties. By this time the body of knowledge of the history of law and the means of accumulating and transmitting that knowledge gave to it all the qualities of a science.

Emel'ianova contends that Soviet scholars like N. P. Zagoskin are incorrect when they deny that such a science existed in Russia in the last half of the nineteenth-century. There had been such an accumulation of historical-legal data by the beginning of the eighties that there were even proposals to create historical-legal sections of juridical faculties. This growth was reflected in the abundance of historical-legal courses in Soviet universities in the first years after the revolution.

This conclusion still does not answer the second question posed by this study: Did this general history of state and law serve as a precedent for the contemporary science of the same name? Emel'ianova believes that what took place in the nineteenth century was undeniably a progressive development during a generally progressive period. The reaction of the 1880s prevented a fuller development of these tendencies. In the Russian universities, courses on the history of the state and law were taught according to the views of individual scholars and had no connecting system. The author believes that bourgeois legal and political sciences had reached the limits of their development during this period. Both their scientific and their practical development could only be achieved under the guidance of Marxism, which revealed fully the true materialist basis of all systems of state and law.

JOHN ATWELL
Hollins College

NORMAN M. NAIMARK. *Terrorists and Social Democrats: The Russian Revolutionary Movement under Alexander III*. (Russian Research Center Studies, number 82.) Cambridge: Harvard University Press. 1983. Pp. viii, 308. \$25.00.

No revolutionary movement in late nineteenth-century Europe enjoyed such vitality and received such widespread support as that in Russia. Terrorism first became a serious threat to state power when the People's Will party set out in the late 1870s to assassinate the tsar; by the late 1890s, under-

ground social democratic groups had developed into an important revolutionary force and were beginning to mobilize and direct a working-class movement that would play a key role in the revolution of 1905. Historians have examined closely the revolutionary organizations and individuals active during these two major periods. The intervening years have received little attention. The author of the work reviewed here has set out to fill in this gap.

Using the voluminous records of the tsarist police as his principal source, Norman M. Naimark has compiled a detailed record of the activities of the radical groups that emerged in the 1880s. He considers the work of the emigres, among whom were eminent intellectuals, less important to the survival of the movement and to the penetration of Marxist ideology in Russia than the work of thousands of obscure radicals active in urban centers throughout the country. His "perspective of the underground" at times provides a vivid account of the ardor and commitment of these idealists and martyrs, in his opinion united, despite ideological differences, by the early 1890s by a "consensus . . . that the time had come to create a democratic, parliamentary Russia" (p. 7). At other times, however, his revolutionary chronicle becomes a tedious story of insignificant groups and unsuccessful ventures best forgotten, whose telling leads him to omit or to gloss over significant problems of interpretation and analysis.

The author considers the 1880s a time of gradual transition in ideology and tactics. He rejects the opinion that at some point "populism" was suddenly and miraculously replaced by Marxism, emphasizing both the continued attraction of the People's Will as a "symbolic focus" for the revolutionary movement and the readiness of radicals to resort to terrorism in the absence of any legal means of political agitation. Several pages are devoted to Aleksandr Ul'ianov and the Terrorist Faction (which he persistently mistranslates as "Fraction") but unfortunately there is no discussion of Vladimir Ul'ianov's (Lenin's) interest in populism following his elder brother's arrest and execution in 1887. Naimark demonstrates—convincingly, in this reviewer's opinion—that throughout those years "social democrats and narodovol'tsy [followers of People's Will] worked harmoniously" in their common struggle against the state (p. 77). They had good reason to do so, for with depressing regularity the ranks of the revolutionaries were decimated by arrests. The author makes clear that widespread support, particularly among students but increasingly as well among skilled workers, provided new recruits to replenish the meager forces of the revolutionaries. Still, one would have to conclude that throughout the 1880s the police held the upper hand.

In view of the limited impact of the revolutionary movement in the period under study, it is disappointing to this reviewer that the author did not explore social and psychological dimensions important to an understanding of the character of Russian radicalism. His brief explanations of recruitment of young radicals are distressingly simplistic, and he appears unaware of the recent literature on the student movement and radicalism during Alexander II's reign. He suggests at one point that certain terrorists were "social misfits" (p. 109), but offers no further psychological insight into the terrorist mentality. In sum, this book provides interesting material on a period that appears, despite his findings, an interlude in the history of the Russian revolutionary movement.

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N. G. KOROLEVA. *Pervaya rossiiskaia revoliutsiia i tsarizm: Sovet ministrov Rossii v 1905–1907 gg.* [The First Russian Revolution and Tsarism: The Council of Ministers of Russia in 1905–07]. Moscow: Nauka. 1982. Pp. 182. 1 r. 40 k.

This monograph, by Soviet historian N. G. Koroleva, is devoted to a study of the history of the formation and activities of one of tsarist Russia's highest government institutions, the Council of Ministers, during the crucial years 1905 to 1907.

Koroleva's book focuses on two points: first, the question of executive power in the system of the reformed state administration and, second, the basic directions of governmental policy during this era. The former area is the focus of her excellent second chapter on Witte's "cabinet" and its "punitive" measures against the revolution—a chapter that is approximately a third of the book in length and is highly descriptive rather than analytical. In the latter area she undertakes to show, in her long third chapter, the class nature of tsarism's internal policies—policies by which it tried to suppress the revolution by renewing itself from "above" and without making any basic change in either its social bases or its long-term goals. In both cases, the author demonstrates the graphic changes that did in fact take place in the structures of tsarism's central apparatus.

Of considerable interest aside from her arguments, however, is the author's introduction, in which she reviews both Western (that is, American and British) and Soviet historiography on this period. As might be expected, one of the main criticisms that she levels against contemporary "bourgeois" historiography is directed at its conceptual and analytical assumptions. These she identifies as the

habit of viewing the autocracy as "independent of class" and the depiction of "westernization" and "modernization" efforts as peaceful paths to the "reform" of Russian society (as opposed to the official Soviet view of the era as one of revolutionary struggle by the workers for complete social and economic freedom [p. 8]). Among the villains are a number of Anglo-American historians who range from Spector to Mehlinger and Thompson to Adams to Hosking, to name but a few.

Although Koroleva's approach is interesting, her efforts to force Western historiography onto such a Procrustean bed fails to take into account real differences of approach, emphasis, and interpretation. In addition, although Koroleva's work was published in 1982, there is little evidence of its familiarity with any Western monographic materials published after 1975.

Finally—and this is the major disappointment—despite some interesting treatment of the Council of Ministers, there is but little indication that the work is based on the rich Soviet archival materials on this period. Instead, what it presents is a reworking, albeit with a vague recognition of bourgeois Western historiography, of a rather traditional polemical line.

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LAURA ENGELSTEIN. *Moscow, 1905: Working-Class Organization and Political Conflict*. Stanford: Stanford University Press. 1982. Pp. viii, 308. \$29.50.

Lenin once observed that a revolutionary crisis is necessarily two-sided. The masses may be unwilling, but the ruling classes must also be unable to uphold the Old Regime. Previous studies of Russia's 1905 revolution have paid more attention to the second factor. In this well-crafted monograph, Laura Engelstein attempts to redress the balance by describing revolutionary unrest among Moscow's workers.

Engelstein sees Moscow workers as essentially similar to Western European workers of 1848—a class "distinguished by its mixed social complexion and by the radicalism of its most stable and most socially privileged sector." Economic and political discontent was most pronounced among artisans in such trades as printing. In factories, a skilled minority provided leadership in times of unrest; the unskilled masses remained on the sidelines for much of the year and tended to regard political issues with indifference.

The heart of the book is a narrative of events from September's uncoordinated "economic" strikes to December's short but bloody uprising. During this time, the center of labor unrest shifted from artisan workshops to factories, from trade unions to

councils based in factories and neighborhoods. The goals and slogans of early autumn were left-liberal, drawn up by a coalition that included white-collar professionals and industrialists. The "days of freedom" that followed the tsar's manifesto of October 17 saw an unprecedented mobilization of workers and a shift of support toward socialist parties. In early December a renewed general strike drew support from eighty thousand Moscow workers, more than twice as many as in October, but in the face of heavily armed troops they could offer only brief resistance. In the course of a week's street-fighting some seven hundred civilians were killed and two thousand wounded.

Engelstein depicts the October concessions as a missed opportunity to "build a political system truly responsive to the country's social needs." The government, whose vacillation and ineptness had brought the nation to a crisis, is blamed for forcing workers and socialist parties into a hopeless insurrection. In crushing worker autonomy, the government also checked the workers' integration into the urban community.

With limited archival access, Engelstein has had to rely heavily on published memoirs, especially those of worker activists. Unfortunately, this articulate minority was not without prejudices, and the behavior of the semipeasant masses may not be fully illuminated in its writings. Although artisans were generally in the forefront of unrest, the "unenlightened" factory workers had a way of turning up unexpectedly and stealing, if not the whole show, at least a few crucial scenes. Until November 1905, textile and other factory industries were underrepresented in strikes, but, when the barricades went up, Red Presnia, home of the enormous Prokhorov cotton mill, became the locus and symbol of insurgency. It was a symbol less of the revolution that was ending than of one that was yet to come, and Engelstein's methodology and conclusions are more appropriate to the former than the latter. One finishes her lucid, thoughtful book feeling that there are other mysteries still to be unraveled.

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CASPAR FERENCZI. *Aussenpolitik und Öffentlichkeit in Russland, 1906–1912*. (Historische Studien, number 440.) Husum: Matthiesen. 1982. Pp. 328. DM 84.

MANFRED HAGEN. *Die Entfaltung politischer Öffentlichkeit in Russland, 1906–1914*. (Quellen und Studien zur Geschichte des Ostlichen Europa, number 18.) Wiesbaden: Franz Steiner. 1982. Pp. x, 402.

These books focus on legal political activity and debate by the press, the legislature, and the political

parties; that is, on "the arena where the interests of society are mediated among each other and vis à vis the state" (*Die Entfaltung politischer Öffentlichkeit in Russland*, p. 4), the point of mediation between state and society" (*Aussenpolitik und Öffentlichkeit in Russland*, p. 37). Both Caspar Ferenczi and Manfred Hagen believe that it is necessary to transcend the customary dichotomy between a rigid state and a hostile society, to inquire into the degree of interaction between the two sides, and to assess the results. Both works, therefore, are contributions to the discussion over the prospects of the Duma monarchy, Russia's "constitutional experiment," before the coming of war.

After a comprehensive examination of the role of the Duma, parties, and press, Ferenczi concludes that the discussion and making of foreign policy provided too weak a basis for the creation of a lasting consensus and could only make a very limited contribution to internal stabilization. What is more, he sees collaboration and agreement between the political public and the policy makers as having broken down even earlier over external than domestic questions and, therefore, places the first signs of the foundering of the constitutional experiment in 1910. Whether one accepts his thesis or not, what he has to say about Russian crisis management and nationalism, the weight given to foreign affairs by the parties, and the evolution of the country's course in foreign policy is compelling and deserves close attention.

Hagen, in his more ambitious study, disavows any intention of taking part in the dispute between "optimists" and "pessimists," of calculating what tendencies might have prevailed in the absence of war, and of judging events in the light of 1917. His declared goal is by a conscientious determination of reality to meet the requirement that historical situations be presented with all their future potentialities—"in ihrer Zukunfts Offenheit" (p. 10). Despite such disclaimers and the cautions and reservations with which the book's findings are advanced, optimists are likely to claim its author as one of their own.

The very title would seem to justify their doing so. What does *Entfaltung* mean but unfolding, development, growth? In fact, much persuasive evidence is brought of growth (in depth as well as breadth) of processes and institutions that, in spite of efforts at repression and restoration, preserved the gains of the revolution and promised their expansion: the appearance of autonomous forms and forums of social communication; the state's involvement in political dialogue; a remarkable degree of freedom for the press to inform, discuss, and criticize; the vitality of party-political life, notwithstanding external restrictions and internal dissension; the growing importance and effectiveness of the Duma in the area of social, economic, and educational reform

and as a bridge across the gulf dividing the masses and the classes. Hagen recognizes that the Council of State seriously inhibited the "unfolding" of constitutionalism (p. 338) but appears at the same time to agree with a contemporary observer that parliamentarism was taking root and gaining strength.

The opening fourth of his book—on society, its divisions, dynamics, and interest groups—also presents a picture of movement, flexibility, openness, and improvement in the social and economic spheres. It is less well documented, more impressionistic, however, than the later chapters. That in these spheres proof, particularly of a definitive quantitative kind, may be lacking should not lead to the neglect of such imperfect knowledge as we do have.

Thus, when the peasants' hostile reaction to collectivization is related to changes for the better that had taken place in their lives between 1905 and 1914, it is done on the basis of limited and uncertain testimony. The memoirs cited as unanimous support for the assumption of rising living standards and changed attitudes are more equivocal in my reading than the author's and could, moreover, be matched by others yielding different results. Both belong to the category of what (on p. 105) are called "subjective recollections." More objective data are available. P. R. Gregory (*Soviet Studies* [1972]) shows that per capita income, which had in 1860 been half the average of the major Western countries, fell to one-third in 1913; that the death rate in that year was double the West European average; and that infant mortality declined only slightly between 1867 and 1911. These figures tell a lot about the countryside, where most Russians still lived and worked.

Twelve pages on the working class contain pertinent reminders of the value of comparison with other countries. Those who make causal connections between the misery in many of Russia's proletarian quarters and the country's political fate are advised to look at the chronic poverty and abuses suffered by factory hands in Britain and at the long and bitter strikes of Pennsylvania miners and English transport workers. When it comes to the trade unions and the considerable scope they are said to have won for their work at the local level, the comparative dimension is lost. A glance at German labor's range of organizations and activities might have shrunk the significance of Russian gains. Since many Russian unions were closed or denied registration, they were losing strength and numbers after 1907 and could not help to integrate workers into society. The Petersburg bias deplored by Hagen finds an antidote in the research of V. E. Bonnell and Diane Koenker on Moscow, which would have lent greater concreteness to this section.

There is looseness also in the assertion that

"countless" Jews lived outside the pale of settlement and in some provinces almost monopolized the editorial offices of newspapers. To say that Jews were "in principle" restricted to "several" Western provinces is less reflective of reality than the census of 1897, which showed 96 percent of them living there (pp. 88–89). Incidentally, they were the *only* inhabitants of Finland, Bessarabia, and the Western and Baltic *gubernii* whom the law defined as *inorodtsy*.

Neither these criticisms nor the book's frequent complaints about the shortcomings of other work should obscure the solid achievements and many insights contained in the bulk of its pages. They are the most detailed and sophisticated overview we have of the structure and stresses, the rules and practices of Russian political life between 1906 and 1914.

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ISRAEL GETZLER. *Kronstadt, 1917–1921: The Fate of a Soviet Democracy*. (Soviet and East European Studies.) New York: Cambridge University Press. 1983. Pp. xii. 296. \$44.50.

As the author of this interesting study points out, there is already a rich literature in English on the Baltic Fleet and Kronstadt with which the present work inevitably overlaps. But Israel Getzler has a specific purpose in focusing on Kronstadt alone and carrying the narrative through the Civil War years: in 1917 Kronstadt developed a unique revolutionary culture, which from the very first lived out the idea of "All Power to the Soviets," a slogan understood to mean a locally based, nonparty direct democracy and an idea that was foisted on an unwilling Provisional Government and soviet leadership. The Kronstadters lived their own life, keeping the ships and port in trim, the streets clean, and public services going without reference to higher authority nominally present in the pathetic figure of the government commissar, Pepeliaev.

Through a closer scrutiny of the sources, Getzler has reconstructed more fully than heretofore the peculiar world that was Kronstadt. Out of the violent upheaval of February 1917, during which more than forty officers were killed and many others imprisoned, the Kronstadters revealed not only an intense class hatred but also revolutionary discipline and pride and a sense of proprietorship toward the very idea of soviet democracy. Getzler demonstrates convincingly that these were self-generated attitudes and not simply creations of Bolshevik or anarchist inspiration, a myth already current in 1917 and enshrined in both Western and Soviet historiography. The anarchist influence is demon-

strated as far less than we have been led to believe, and the Bolsheviks are shown to have become a significant force in Kronstadt politics in the crises of April and July because they articulated the Kronstadt experience against the mainstream coalition politics supported by the leadership of the Petrograd Soviet: in fact, the Bolsheviks never acquired a firm majority in the Kronstadt Soviet and at no point exercised a monopoly of its leadership. They worked their will mainly by raising crowds on Anchor Square. The first elected Executive Committee of the Kronstadt Soviet was a *mélange* of all socialist factions, mainly the left internationalists of the respective parties along with a "nonparty" group led by Anatolii Lamanov, who epitomized the Kronstadt mystique. He was a reconciler and facilitator rather than a bitter partisan, and, in Getzler's account, remains the pivotal figure linking the Kronstadt of 1917 with the Kronstadt of 1921. His group evolved into the Socialist Maximalists, who managed to preserve the ideology and memory of pure soviet democracy through the Civil War and the regime of commissars.

Getzler's most significant conclusions are that the Kronstadters' autonomy was undisturbed by the October Revolution (since they already practiced soviet democracy and their services were indispensable to the Bolsheviks as praetorians), that the Bolsheviks assumed direct central control only in the summer of 1918 in response to the Left SR uprising, and, finally, that, despite the rigid party-state controls of the Civil War years, the Kronstadt spirit and even its informal leadership remained intact, so that the episode of 1921 under the banner of "Soviets without Communists" becomes perfectly credible and logical. He revises the accepted view of a considerable turnover in personnel and social composition, particularly of the naval contingent, so that in 1921 we are not dealing with newly recruited peasant types, but with the same core of ship and shore sailors who had experienced the 1917 revolution (the *Bolsheviks*, however, had been significantly drained off to staff central institutions). Peasant grievances still stand out in the affair of 1921, but the Kronstadt of 1917 was predominantly neither worker nor peasant, but a nicely balanced amalgam of rural and urban Russia with a substantial group of artisans, petty tradesmen, and occasional laborers who bridged the two worlds. Getzler has rendered an inestimable service in telling this story so well.

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G. I. IL'INA. *Kul'turnoe stroitel'stvo v Petrograde, Oktiabr' 1917–1920 gg.* [Cultural Construction in Petrograd, October 1917–20]. Leningrad: Nauka. 1982. Pp. 238. 1 r. 40 k.

Lenin wanted to keep the good and discard the bad of the "old culture" and he fought against those who wanted to discard it all. The trouble was that in the end, Lenin and his cohorts decided just what was "good." This is one of the major themes in G. I. Il'ina's chronicle of cultural life and institutions in Petrograd during the revolution and Civil War. Dealing separately with theater, graphic arts, music, festivals, and film, its underlying themes are preservation versus iconoclasm, the politics of art, and propaganda through monuments and festivals. As to the first, the Bolsheviks contained the brief flurry of destruction, coopted some of the old artistic intelligentsia, collected and nationalized art treasures (partly in order to avoid illegal outflow), eliminated the world of smaller beauties and private splendors (personal and estate collections), concentrated art and treasure into large museums, and opened these—almost for the first time—to the common people.

Russian theater (including, in Soviet usage, ballet and opera) was the first arena of the controversy over autonomy. The managers, impresarios, and artists wanted freedom of repertoire and direction; they also wanted protection and patronage (including rations and apartments in the hungry winters of the Civil War period). And they disagreed violently among themselves. (The normality of theatrical life perceived by so many memoirists during the revolution is belied in Il'ina's account of titanic struggles behind the scenes of the major dramatic establishments.) The problem, which had already emerged under the Provisional Government, was solved—reluctantly at first and then with decisiveness—by nationalization of theatrical life. The result was large-scale emigration of many famous artists (especially in ballet), a retention of classical and Russian national repertoires in the big Petrograd theaters, and the parallel emergence of a revolutionary theater culture (plays about the European radical tradition, futurist pieces, and a network of "proletarian culture" studios and workshops). The angry and frank exchanges between Commissar of Culture Lunacharskii and the older theater magnates are recorded here with great vividness.

The third major theme is the impact of Lenin's famous "scheme for monumental propaganda" in the former capital. This scheme, inspired by Campanella's *City of the Sun*, was to remove the offensive statues of the old ruling circles, replace them with monuments to figures of the revolutionary tradition (Robespierre, Lassalle, Perovskaia, and so on), and festoon the city during festivals with mottoes and legends of humanistic and radical content. Il'ina's account of the first festivals in Petrograd shows that there, as in Moscow, the scheme served to decorate the city, put some unemployed laborers to work, coopt artists, and draw the line between avant-garde

taste and that of the masses and their leaders. It also describes how early elements of spontaneity and carnival gave way to organization, control, hierarchy, and routine in public festivities.

Il'ina's book is good on institutional details, chronology, lists of people, and the state of confusion and controversy in the early period of the revolution. But in failing to use recent Western and Soviet works on related topics, particularly the books of Sheila Fitzpatrick, Harold Segel, Lars Kleberg, Richard Taylor, Robert Williams, John Bowl, Nils Åke Nilsson, and A. I. Mazaev, she fails to raise her detailed research to the level of an interpretive monograph. In failing to make extensive use of the memoir literature, she is forced to look at the world from within the arts community, but never into it from the outsiders' perspective. It is a useful survey of the main components of the story, but it lacks the pathos, the color, the richness that any cultural history of Petrograd in those sad but euphoric years ought to provide.

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STEPHAN MERL. *Der Agrarmarkt und die Neue Ökonomische Politik: Die Anfänge staatlicher Lenkung der Landwirtschaft in der Sowjetunion, 1925–1928*. (Studien zur Modernen Geschichte, number 25.) Munich: Oldenbourg. 1981. Pp. 530. DM 110.

The reasons for Stalin's collectivization of Russian agriculture after 1928 are among the most important issues of Soviet history. Even today, and in spite of convincing challenges, the conventional explanation in Western historical literature stresses the unworkability of the NEP (New Economic Policy) system of peasant smallholdings and portrays Stalin as being virtually obligated to collectivize in order to build a system in which the state could secure adequate income transfers from agriculture for use in industrial development. Stephan Merl's book is, therefore, a work of major importance. It argues—and, more importantly, it demonstrates—that there was no economic basis in the Soviet government's experience with agrarian policy in the 1920s to justify agricultural collectivization.

Merl concentrates on the period of Soviet agricultural policy from 1924 (the policy of "Face to the Village") to the beginning of collectivization in 1928. He focuses simultaneously on government agricultural policies and peasant responses to those policies, setting both in a thorough analysis of the changing economic and social life of the village. He concludes that the Soviet government had quite adequate instruments for the management of agriculture—especially peasant production and marketing decisions—within the context of a program

stressing industrial modernization. The peasants reacted sensibly and flexibly to government policies, and, with scant government support, continued to make heavy contributions to the industrialization drive. But a faulty pricing policy discouraged agricultural production and sale by setting delivery prices too low, creating shortages to which the state responded with periodic campaigns of forced grain collection. These actions alienated the peasantry and further disrupted grain production and sales. The government then blamed the problem on the hostility of the "rich" peasants and trading organizations and initiated forcible collectivization, a struggle of the state against the independent farmers.

Merl's main conclusion, then, is that the agricultural system of the 1920s provided impressive transfers of capital for industrial development, though agricultural investment was neglected. Under collectivization, agricultural production declined, as did capital available for transfer from agriculture to industry, even though "less favorable terms of trade" (that is, heavier exploitation) were imposed on the agricultural producer. In purely economic terms, the continuation of NEP would have provided a far better foundation for Soviet industrialization than collectivization.

It would appear that future writing on Soviet history should present the decision to collectivize agriculture as an amalgam of ideological predilection and the consequences of poor economic management. And those who study the Soviet experience as a model of industrial development will want to look more closely at the relationship of agriculture and industry in that model, especially its roots in government policies of the NEP era. For both purposes, Merl's fine book will be indispensable reading.

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S. A. TIUSHKEVICH *et al.* *Istoriia vtoroi mirovoi voyny, 1939–1945*. Volume 12, *Itogi i uroki vtoroi mirovoi voyny* [History of the Second World War, 1939–45. Vol. 12, Results and Lessons of the Second World War]. Moscow: Voennoe Izdatel'stvo Ministerstva Oborony SSSR. 1982. Pp. 494. 2 r. 80 k.

The twelfth and final volume of the Soviet history of the Second World War, by S. A. Tiushkevich and others, is devoted to summing up the results of the war and presenting its lessons. The work is intended not only to demonstrate the superiority of the Soviet system to that of its wartime foes but also, more importantly, to convince the reader that the superiority of the Soviet system continues into the present.

The reader is also constantly reminded that, although the shock troops of imperialism—Germany, Italy, and Japan—were defeated in 1945, the aggressiveness of today's imperialists (the United States and its allies) continues to increase. The reader is warned to be vigilant and to struggle uncompromisingly with the ideology of war. "The victory over fascism, its results, and lessons are not only the past but also the present and the future of humanity" (p. 10).

The first part of the work, concerned with the sociopolitical results of the war, repeats the now-familiar theme that imperialism was guilty of starting the war. It encouraged Germany with arms, loans, and equipment. Its hope was that Hitler would be content to move east. When this plan backfired and Hitler turned west, there was no force in the capitalist world that could halt him. Only the Soviet social system and the mighty force of Soviet patriotism had the strength to defeat him.

In part 2—on the economic results of the war—we are told that the Soviet economy was the most effective arsenal of victory. Bourgeois economics operated in the interest of monopolies while the ruling classes strove to make the laboring people pay for the war. A significant portion of American resources and economic effort was devoted to ensuring its economic and political leadership in the postwar period (p. 185).

The Soviet general staff receives perhaps the best notices in part 3, which deals with direction of the military effort. In a work that emphasizes the collective aspects of the Soviet efforts, the four wartime Chiefs of Staff are named. One of the few critical comments on Soviet performance, however, notes that many questions about the high command had not been settled by the beginning of the war. Although not stated, this was certainly a responsibility of the general staff. Part 3 concludes that the war confirmed the advantages of the Soviet method of directing the armed forces "based on an advanced social system, the high moral and combat qualities of the personnel, and scientific methods of understanding military phenomena" (pp. 352–53).

The title of the first chapter of part 4, "The Radical Change in Correlation of Forces in the World," is an accurate indicator of the Soviet point of view on the influence of the war on the postwar balance of power. The appearance of the world socialist system is described as the greatest (*krupneishii*) historical event since the October Revolution. The authors' confidence in the Soviet position in the world is somewhat subdued when they inveigh (in accordance with the current Soviet line on peace and disarmament) against the dangers of a future nuclear war that might be unleashed by the imperialists. This doubt is only temporary. The reader is soon told (p. 457) that the war has shown that there

is no force in the world that can destroy socialism and halt its irresistible progress.

Volume 12 brings to a close a work that, from the point of view of a Western historian of the Soviet Union, can only be described as disappointing. The twelve volumes add little to what was already known about the Soviet war effort. In some respects, it tells less than the six-volume history of the war published during the Khrushchev regime. In that work, Khrushchev's writings and speeches are frequently cited. In the work being reviewed, Leonid Brezhnev is often cited. One can only wonder who would have been cited as the ultimate authority if this volume had been published a year or so later.

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DAVID ROUSSET. *A Critical History of the USSR*. Volume 1, *The Legacy of the Bolshevik Revolution*. Translated by ALAN FREEMAN. New York: St. Martin's. 1982. Pp. 333. \$27.50.

American college bookstores have the odd habit of putting books on the history of Russia and the Soviet Union in a section called "Marxism." In the past, Western scholarly works in this field were usually anything but Marxist, but recently Marxist interpretations of Soviet history have been appearing with some frequency. David Rousset's *Critical History*, like Charles Bettelheim's *Class Struggles in the USSR*, comes out of a French and Trotskyist intellectual tradition.

Rousset was a French Trotskyist in the 1930s, then an inmate of German concentration camps, and from the late 1940s he was a prominent left-wing critic of the Soviet labor camps. His book reflects this background. A major theme is the emergence of the Soviet bureaucracy as a social class, its alliance with Stalin, and its consolidation as a new, privileged ruling class in the 1930s. Rousset quotes with approval Rakovsky's 1928 assessment that the new ruling class was based on a new kind of property, namely the possession of state power. He argues, further, that "the state in fact belonged as private property to the social corps of the state," that is, to the bureaucracy; and that it therefore became the main function of Soviet law to protect these "property rights."

Rousset identifies Stalinist society as "a state-capitalist bureaucratic society containing powerful concentration-camp deformations," explaining the latter partly in terms of world events (the degeneration of capitalism and, in particular, the rise of Nazism and the German concentration-camp system) and partly as a product of state coercion and violence in the period of Soviet collectivization. Despite the breakup of the Soviet labor-camp em-

pire after Stalin's death, Rousset sees "fundamental dynamic continuity" in the Soviet system from 1930 to the present. For him, as for many brought up in the revolutionary faith, the only real break in Soviet history occurred when the Leninist-Trotskyist revolutionary tradition was overthrown by Stalin at the end of the 1920s.

Although parts of these arguments are interesting, the book as a whole leaves a strong impression of *déjà vu*, since so many of its ideas come out of the familiar Trotskyist canon. The Marxist jargon is sometimes impenetrable, although this may be partly the fault of the translator. Hardly any sources are cited (the notes take up less than four pages at the end of the book), and there are many inaccuracies, undocumented assertions, and statements of opinion presented as fact. Although Rousset juggles the old issues ingeniously, it is hard for him to find new perspectives (except when drawing on personal experience of concentration camps or the Trotskyist movement) because he has no new data.

Rousset rightly sees that the Great Purge of the late 1930s provides him with an interpretive challenge, given his premises that the bureaucracy was a ruling class and that alliance with it was the basis of Stalin's power. Why did Stalin cut down his bureaucratic allies? How did he do it without destroying his own power? In the face of such violent dislocation of the ruler-bureaucracy relationship, how could the society and political system remain (as Rousset argues) essentially unchanged? Some of the more interesting passages in the book—as well as some of the sloppiest and most poorly documented, especially on the numbers of purge victims—deal with these questions. But ultimately Rousset has no convincing answers to them.

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ROBERT F. BYRNES, editor. *After Brezhnev: Sources of Soviet Conduct in the 1980s*. (CSIS Publication Series on the Soviet Union in the 1980s.) Bloomington: Indiana University Press, in association with the Center for Strategic and International Studies, Washington, D.C. 1983. Pp. xviii, 457. Cloth \$25.00, paper \$12.50.

This volume, edited by Robert F. Byrnes, is the first major product of the "Project on the Soviet Union in the 1980s" of the Georgetown Center for Strategic and International Studies. It is intended to provide an up-to-date summation of our knowledge of the Soviet Union—its strengths and weaknesses, successes and failures—past, present, and future. A lavishly funded enterprise, the project brought together virtually a cast of hundreds to consult and

critique in a variety of combinations and permutations, with at least one eye on public relations and impact in Washington.

It might be unkind to suggest that of this mountain of expense vouchers a mouse was born, for it is both a substantial and a respectable mouse. Yet the dominant impression of this collective enterprise—whose contributors are all prominent, knowledgeable, or both—is that the whole adds up to a good deal less than the sum of its parts. True, if this hefty volume is heavy on conventional wisdom, this too may have its uses. Although born under impeccably establishmentarian auspices, the thrust of the book's argument is to describe the Soviet political, economic, social, and military systems as facing serious challenges and difficulties but surely to deny and to disprove, by implication, the scenarios of the impending collapse of the Soviet system that are so dear to some denizens of the nation's capital.

A number of the contributions are well-informed and thoughtful distillations of what American scholars know—or think they know—about the Soviet Union, and inasmuch as this was apparently one of the project's purposes, its sponsors can take comfort in the general soundness of the results. This is true, for instance, of the discussion of politics by Seweryn Bialer, of foreign policy by Adam Ulam, of the Soviet economy by Robert W. Campbell, and of military affairs by Coit D. Blacker. Andrzej Korbowski deals with Soviet policy toward and perceptions of Eastern Europe. In most cases these are what might be considered centrist or mainstream interpretations, which those at either end of the scholarly and political spectrum will be loath to accept. The two most original contributions, underlining the relative weakness of previous American work on Soviet society and social and cultural history, are the essays by Gail W. Lapidus on social trends and by Maurice Friedberg on cultural and intellectual life.

The analysis of Soviet political trends, in line with Bialer's earlier writings, is generally judicious, though others will wish to give greater prominence to elite politics and policy disputes. One might also question the formulaic description of Soviet policy as "the external expansion of an internally declining power" (p. 65). The economics chapter and the general conclusion properly stress the objectively difficult choices and constraints, compounded by the priorities of the Soviet policy makers, as well as the interlocking nature of decisions in different issue-areas.

The discussion of Soviet society focuses on the interaction of several simultaneous trends—the effects of the economic slowdown, the declining opportunities for upward social mobility, demographic trends, the nationality question, and the problems with "civic morale." Important in showing the di-

minishing capacity of the political system to shape the social system, the Lapidus essay documents an interesting change of values, marked by a social conservatism, which is also well illustrated in the essay by Friedberg, who finds the pessimistic mixture of *travail, famille, patrie* more reminiscent of Vichy France than of Karl Marx (p. 289). The discussion of military power and outlook is exceptionally well informed and sober. Most uneven, perhaps, are the chapter on foreign policy and the general conclusion, which fails to offer any novel insights.

Two central questions that most essays make an effort to address concern the prospects for change and crisis. Most regrettable here is the failure to define and standardize the usage of these two terms. It remains uncertain how much change amounts to "significant change"; and although most contributors speak of an increasing severity and complexity of challenges to the system in the 1980s, the reader is not told just when they are likely to add up to a "crisis" (or what a crisis would mean). Moreover, there would appear to be an unresolved contradiction between the argument that, "like all countries at all times" (p. 423), Moscow must face serious challenges in the years ahead and the proposition that, compared both to the past and to other countries, the Soviet Union faces uniquely serious or destabilizing difficulties.

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NEAR EAST

CHARLES ISSAWI. *The Economic History of Turkey, 1800–1914*. (Publications of the Center for Middle Eastern Studies, number 13.) Chicago: University of Chicago Press. 1980. Pp. xvi, 390.

Charles Issawi has provided an extremely useful collection of contemporary reports, mostly European but a few Ottoman, and excerpts from a few modern Turkish and Western studies, regarding the economy of the Ottoman empire in the last century of its existence. It is divided into seven chapters: social structure, trade, transport, agriculture, industry, and finance and public finance, with the author providing useful introductions not only to the chapters but also to their subsections.

This work is a mine of useful information on all aspects of the Ottoman empire. It is especially useful in providing contemporary statistical information often not found in Ottoman sources, such as the local prices of manufactured goods (p. 119), the number of foreign merchant houses in the empire (p. 101), average daily wages of workers (pp. 40–

43), the budgets of individual urban and rural families (pp. 45–50), the number of steam vessels in the port of Istanbul in 1838 (pp. 158–59), the prices of principal foodstuffs in almost every year between 1828 and 1914 (pp. 335–36), and the like. The accuracy of such European reports is, however, open to considerable question, and the information must be used with the same caution that often marks scholarly treatment of Ottoman statistics.

One wonders also why, while including the highly questionable and inaccurate population estimates provided by various foreign reporters (pp. 34–35), Issawi did not include the considerably more accurate statistical information found in contemporary Ottoman publications such as the annual yearbooks (*salname*) published for the empire as a whole as well as for individual provinces, the huge volumes of financial statistics published by the Ottoman ministry of finance in the Young Turk period, called *Ihsaiyat-i Maliye*, the very detailed statistical collection, *Devleti Aliyei Osmaniye'nin 1313 Senesine Mahsus Istatistiki Umumisidir* published by the ministry of trade in 1895, and the 1914 census, published as *Memaliki Osmaniye'nin 1330 Senesi Nüfus Istatistigi*, published during the Allied occupation of Istanbul in 1920. Fortunately, however, these were published recently by Justin McCarthy in *The Arab World, Turkey and the Balkans (1878–1914): A Handbook of Historical Statistics* (1982), which should be considered an essential supplement to Issawi's work.

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ELIE KEDOURIE and SYLVIA G. HAIM, editors. *Zionism and Arabism in Palestine and Israel*. Totowa, N.J.: Frank Cass. 1982. Pp. x, 255. \$42.50.

This collection of ten unpublished essays is a companion volume to *Palestine and Israel in the Nineteenth and Twentieth Centuries* (1982), also compiled by Elie Kedourie and Sylvia G. Haim. Both volumes cover aspects of diplomatic, political, and economic development in Palestine and Israel, and both concentrate on the period of British rule (1918–48). In fact, it would have been preferable to rearrange the two volumes so that the first focused exclusively on the mandate era and the second covered the debate over the British withdrawal from Palestine (1946–48) and aspects of Palestinian-Israeli relations since 1948.

One theme that suffuses the essays is the irreconcilability of Arab and Jewish aims in Palestine. The opening study by Neil Caplan describes the tension in the early 1920s between the Jewish community in Palestine and the first British high commissioner. Even though the high commissioner was Jewish, he

viewed his primary responsibility as balancing the needs and the demands of the two peoples, which prevented him from meeting all the requests of the Zionists for mass immigration, large-scale land purchases, special politico-communal rights, and crack-downs on the Arabs. The tension between the two communities peaked in the late 1930s. Shai Lachman describes the Arab revolt that tried to halt the Zionist inflow. The revolt was spearheaded by Sheikh Izz al-Din al-Qassam, a Syrian-born preacher in Haifa, who synthesized Islam and nationalism in his teachings and whose militant bands formed the core of the commanders of the revolt. The revolt, however, could not stem the Zionist tide and degenerated into inter-Arab strife. The efforts by the Arab rulers to assist the Palestinians diplomatically—and the legitimization of the rulers' involvement by the late 1930s—is delineated by Aaron S. Klie-man. This essay is complemented by a brief study of the propagandistic and military assistance lent to the Palestinians in the late 1940s by the Egyptian Muslim Brethren, written by Thomas Mayer.

By the mid-1940s the British were seeking a way to extricate themselves from Palestine, whether through establishing a unitary state or acquiescing in partition. Their complex interactions with the Zionists, Arabs, and Americans are chronicled in painstaking detail by Joseph Heller and Amitzur Ilan and through anecdotal excerpts from the diaries of two members of the Anglo-American Commission and the testimony of a Syrian politician, compiled by Allen H. Podet. The continued inability to reconcile Zionism with Palestinian rights is analyzed perceptively by Yael Yishai in her study on the internal contradictions faced by the Mapam party from 1948 to 1954 when some members sought to admit Arabs as full members.

The two remaining essays do not fit as naturally into the volume as the others. Yaacov Ro'i analyzes the ways in which the Soviet Union linked its policy toward Jewish emigration to its stance on the Arab-Israeli conflict, during the period from 1954 to 1967. Ibrahim A. Gambari, a Nigerian scholar, discusses Nigerian-Israeli relations up to the October 1973 war when Nigeria broke diplomatic relations with Israel. His analysis of different attitudes among Christian and Muslim regions in Nigeria, the negative effect of Israel's support for the Biafran secessionists, and the Nigerians' growing frustration at Israel's continued occupation of Arab land provide unique insights into the factors affecting African attitudes toward Israel.

Like all edited volumes, the essays are of uneven quality. The collection does have some high points, notably the essays by Lachman, Ro'i, Gambari, and Yishai. Others cover more familiar ground. It was also unfortunate that no essays by Arab scholars were included. Nevertheless, the volume is useful

for historians specializing in Palestinian or British diplomatic history and should be included in research library collections.

ANN M. LESCH
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ERIC DAVIS. *Challenging Colonialism: Bank Misr and Egyptian Industrialization, 1920–1941*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1983. Pp. xv, 232. \$23.50.

Founded in 1920 as the only Egyptian-owned bank in an economy dominated by Europeans, Bank Misr spearheaded an impressive attempt at capitalist development and industrialization in the Third World. Until its bankruptcy in 1939 it provided the capital for over a score of commercial and industrial companies of the "Misr Group." The Misr Spinning and Weaving Company was the most successful of these.

A remarkable entrepreneur named Tal'at Harb led Bank Misr, but it was the financial support of the rural landed class that made the venture possible. These village notables owed their wealth mostly to expanded cotton cultivation after the British occupation of 1882. Stung by a financial crisis in 1907 and British economic policies during World War I, they resolved to have more say in their country's economic and political fortunes. Bank Misr came into being in the wake of the nationalist uprising of 1919. For its first few years support of the bank became an article of faith among the student shock troops of the nationalist movement.

Bank Misr's need for some degree of national and international economic stability, however, alienated it from those nationalists who pushed hard for immediate and total independence from Britain. During the Great Depression the bank even abandoned its principle of relying only on national capital and entered several joint-venture schemes with British firms. The bank's need for tariff protection and subsidies for certain of its companies forced it to buy the support of parliamentary politicians and government bureaucrats; unsecured loans and ill-deserved appointments to its boards of directors were the price. Bankruptcy ensued in 1939. The government eventually rescued the more profitable Misr enterprises, but only at the price of the dismissal of Tal'at Harb and his associates and the abandonment of the bank's goals for industrializing Egypt.

Eric Davis uses a political economy framework, seeing the gradual absorption of Egypt into a Western-dominated world market as the central theme of nineteenth-century Egyptian history. He insists, however, that dependency/world-systems concepts cannot account for the bank's success at development in the "periphery," while Leninist-inspired

theories of imperialism cannot explain the eventual failure of this national capitalist attempt to replace the dominant Western capitalists. The argument is intriguing, but it may depend on depicting both schools as being more monolithic than they are—or at least need to be.

The author exploits an impressive array of English and Arabic sources, drawing on British and Egyptian national archives, the private papers of Tal'at Harb, the Arabic periodical press, and interviews with Bank Misr officials.

By situating the story of Bank Misr firmly within the context of evolving Egyptian social structures and developments in the world economy, Davis has made an excellent contribution to Egyptian history. Anyone interested in the problems of development in the Third World could read this well-conceived, carefully researched, and clearly written study with profit.

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ROSEMARIE QUIRING-ZOCHE. *Isfahan im 15. und 16. Jahrhundert: Ein Beitrag zur persischen Stadtgeschichte*. (Islamkundliche Untersuchungen, number 54.) Freiburg: Klaus Schwarz. 1980. Pp. 340.

Iran is a land of urban networks. Geography has divided the Iranian uplands into distinct regions within which the availability of water has helped determine patterns and types of settlement. Typically, each region has had one dominant urban center, the setting of which has remained relatively unchanged over long periods of time. Thus the major urban centers of today—Tehran (medieval Rayy), Tabriz, Mashhad (Tus), Hamadan, Shiraz, Yazd, Kirman, and Isfahan—are also the great cities of the middle ages.

Despite the importance of the city phenomenon in Iran, the study of Iran's urban history has not progressed very far although the work of Eugen Wirth, Heinz Gaube, Jean Aubin, and Iraj Afshar has begun to provide an appropriate methodology for the study of the Iranian city and the necessary data to work from.

There is, however, one Iranian city, Isfahan, which has long drawn exceptional attention from scholars and laymen alike. Partly this may be explained by its accessibility to Europeans after the fifteenth century, a time when Europeans were beginning to acquaint themselves with the rest of the world. But what seems to have impressed itself most on European sensibilities and contributed to the memorialization of the city was the magnificence of the scale of urban development there at the end of the sixteenth and beginning of the seventeenth centuries. As a result of this visual impact, the city of

the middle ages as known to us today is in fact the city of Shah 'Abbas, the Isfahan of the seventeenth century. Isfahan's physical appearance before 1600 and even more the internal organization of the city, its society, economy, and regional administration remain particularly obscure today.

The present work by Rosemarie Quiring-Zoche takes a significant step toward augmenting our understanding of the city before the large-scale urban projects instituted by Shah 'Abbas. The author has brought together all the published material pertaining to Isfahan in the two centuries before its transformation and has produced a coherent and comprehensive narrative of the city's political, social, and, to a lesser extent, architectural history during that time.

The study is inevitably shaped by the available materials, which tend to document the activities of soldiers and politicians rather than the working population or even the business community. The work falls more or less evenly into three sections. The first part is a political history of Isfahan in the fifteenth and sixteenth centuries. After the compressed obligatory summary of the prefifteenth-century history, the author narrates the major political struggles and crises that periodically replaced the ruling cadre. Here and there, however, she has found nuggets of information on less usual themes such as public health crises and the evolution of the city's infrastructure. In accordance with the fragmentary way in which such material appears in the Persian sources, however, these bits of information are not related to overall economic or demographic trends, whether regional or world-wide, which may have produced the catastrophes or odd circumstances noticed by contemporary writers.

The book's second section describes the municipal and regional administration as it developed during this period. It notes the city's administrative role, in particular its supervision of water distribution from the Zayandeh River. In general, the work done here is a summary of the work of Hans Robert Roemer, Ann Lambton, Roger Savory, and Vladimir Minorsky.

There then follows one of the two most interesting and valuable sections of the work—a brief but important inventory of public building projects undertaken by leading citizens during the two centuries. This concise discussion of patronage with a map and chart summarizing the author's findings and the following section on the leading families of Isfahan during the period studied are original and significant contributions. In the section on the city's eight leading families, brief biographies are given for fifty-eight prominent members out of which a picture emerges of the career paths of the urban elite and of the interlocking nature of the urban intelligentsia and the municipal administration and

the long and dominant roles played by these families in both spheres. All that is missing is a discussion of the commercial and financial roles of these same families.

The work stands as a useful summary of the sources and of later scholarship on Iran in general and Isfahan in particular for the fifteenth and sixteenth centuries. The bibliography is reasonably complete and the four indexes are excellent.

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ASIA AND THE EAST

SALLY BORTHWICK. *Education and Social Change in China: The Beginnings of the Modern Era.* (Education and Society.) Stanford, Calif.: Hoover Institution Press. 1983. Pp. xviii, 216. \$21.95.

In *Education and Social Change in China*, Sally Borthwick depicts traditional Chinese education and the modern educational system adopted from Japan and the West as mirror images. Modern education, the expression of and contributor to an industrialized society, is characterized by regularization, sequential graded progress, public funding and control at lower levels, child centeredness, and universality. Traditional Chinese education was structured to train an elite leadership through private schools (*sishu*); pace and appraisal of progress were individualized; calendars and schedules were flexible; and, although the government concerned itself with ideological orthodoxy, its role in funding and administrative control was minimal. Transition from one system to another would be difficult, especially since China was slow to evolve an industrialized society compatible with and providing employment for products of the new school system. Yet early in the twentieth century Chinese educators sought to displace the *sishu* with a national educational program to train loyal citizens.

Borthwick's monograph documents the gap between the ideal blueprint developed by educators in Beijing and the real world of schooling in the provinces. She illustrates the continued popularity of the *sishu* staffed by a single tutor teaching the classics. During the first two decades of the twentieth century more pupils were probably enrolled in *sishu* than in the new schools, located principally in urban centers. The unevenness of change in China and the growing gulf between urban and rural are once again dramatized.

Even the new schools were in reality a compromise with tradition. The central government, lacking the fiscal or administrative structure to implement the national plans, could exercise little control.

Short of funds and trained teachers, the urban schools charged fees to an elite training for leadership. Funding and control of local schools in agraria were in the hands of the gentry, who served their own purposes. Frustration over unfulfilled expectations contributed to anti-Manchu sentiment and rebelliousness among youth.

Relying heavily on autobiographies and contemporary periodicals, Borthwick provides valuable detail to substantiate the thesis briefly summarized here. This in itself is a contribution.

What leaves the reader with a sense of unease is a lack of consistency in the argument. Although Borthwick acknowledges that problems of size and communication created a qualitative difference between China and Japan, she still contrasts educational change in the two countries to the detriment of China. At the same time, she points to inadequate administrative and fiscal control by Beijing in explaining difficulties in implementing the national educational programs. Even though she denies necessary dichotomy between the modern and the traditional, she views the attempts to institute a new system as overturning the consensus and coincidence of interests characterizing the old. She also demonstrates, however, that the *sishu* continued to operate.

Perhaps the source of the difficulty is that Borthwick, along with many other scholars, starts with an assumption of failure on the part of China before 1949. The study thus becomes a search for answers to negative questions. The value of the work lies not so much in causal explanations of China's inability to implement a national educational system during the early twentieth century as in its depiction of the actual functioning of the educational programs in the *sishu* and the so-called new schools.

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YAO MING-LE. *The Conspiracy and Death of Lin Biao.* Foreword by STANLEY KARNOW. New York: Alfred A. Knopf. 1983. Pp. xvi, 231. \$13.95.

Published about the same time as Hitler's diaries, *The Conspiracy and Death of Lin Biao* was scheduled to appear in Canada, Britain, Italy, and other countries, although its authenticity awaits verification. Lin Biao, defense minister, was Mao Tse-tung's heir designate. Mao and Lin made great contributions to the political and military arenas respectively of the People's Republic. During the Cultural Revolution (1966-76), the army played a key role. Lin managed to have twenty of twenty-nine provincial parties under army control. He then challenged his superior. Mao insisted that the party control the army; Lin contended that the army command the party. Af-

fectured by egomania and megalomania the heroes could no longer tolerate each other but were compelled to go to the extreme. Lin, not as smart as Mao, devised assassination plots, a repeated occurrence in Chinese dynastic successions.

This book by Yao Ming-Le vividly narrates such schemes. Lin planned to provoke a short war with Russia to persuade Mao to move to an emergency bunker in the Western Hills. Lin would gas Mao, seize power, and negotiate peace with USSR against the establishment of U.S.-China relations. In March 1971 a coded "Project 571" (the number, read "wuch'i-yi" in Chinese, is a homonym for "armed uprising") was drafted. It was designed to kill Mao by blowing up his special train on his return to Peking from an inspection tour. Mao apparently learned of the plot; he upset the plotter's timing by shortening his stay in Shanghai and Nanking and omitting other stops so that he returned to Peking at dusk on September 12, 1971. That evening Lin Biao, his wife Ye Qun, and his son Liguao, who were then at Beidaihe about 200 kilometers from Peking, recognizing their defeat, boarded an army Trident jet at Shanhaiguan airport and flew toward Russia. The Trident crashed in Mongolia; the Lins and six others were killed.

The new thesis of the *Conspiracy* is that on the evening of September 12 Mao invited Lin Biao, Ye Qun, Chou En-lai, Wang Dongxing (Mao's security chief), and others to a dinner party at Mao's villa in the Western Hills. The wonderful banquet included "imperial wine sealed in its original Ming dynasty porcelain vessel 482 years earlier," and "tendons of tiger shot recently in Manchuria." About ten o'clock most guests left; the Lins were urged by Mao to stay longer. At eleven o'clock a 60mm rocket was heard throughout the villa. Lin Biao and his wife and their car were destroyed.

Some comments are due. It is a fact that Mao returned unexpectedly to Peking at dusk on September 12. An immediate elaborate dinner for Lin would have been impracticable. Moreover, the Lins were then at Beidaihe, whence their daughter, Lin Doudou (borne by his first wife), secretly reported their impending flight to Chou En-Lai, who apparently had other information, including telephone tapes. He immediately ordered that no planes could land or takeoff from any airport in China from September 13 to September 15. Chinese diplomats in Mongolia, who inspected the wreckage on the spot, reported to Chou that nine burned bodies were on the plane. On September 30 the Soviet Tass agent confirmed the fact. This official explanation of Lin Biao's death, although unsatisfactory, makes better sense than the dinner tragedy. The new thesis is open to doubt.

Other stories are well known to the students of contemporary China. Even so-called top secret docu-

ments have been published in Hong Kong and Taiwan, and some have been translated into English, including the "571 Project." It was printed for "domestic reference" by cadres, some of whom smuggled copies out for sale.

The Conspiracy and Death of Lin Biao is ambiguous. The publisher declares that Yao is a pseudonym; if he authored the Chinese version, we would like to see the original text in order to check doubtful points. For example: *Chuan da wen-jian*, "document-reading sessions" (p. 6) should be "documents in circulation." "Mao had slept all the way from Nanjing to Henan province" (p. 189) shows the author's geographic ignorance of the Peking-Shanghai railway, which does not pass through Henan. "On September 14, 1971," somebody "was taken from Dalian to Beijing" (p. 14)—this contradicts the ban on all flights from September 13 to September 15.

The book presents little historical background, interpretation, or analysis but contains instead puerile plots and sensational gossip about the amorous affairs of Ye Qun, Jiang Qing, and Lin Liguao. Spies of Mao and Chou infiltrated Lin's harem. Even the writer of the introduction, Stanley Karnow, "cannot vouch for the authenticity" of the book. This reviewer, an abortive Lin Biao biographer, considers this book an irresponsible fiction published for wide circulation, but the mystery involved may become a research topic for future historians.

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DOROTHY ROBINS-MOWRY. *The Hidden Sun: Women of Modern Japan*. Foreword by EDWIN O. REISCHAUER. Boulder, Colo.: Westview. 1983. Pp. xxii, 394. Cloth \$32.50, paper \$13.95.

This narrative account of women's place in Japanese history and society—and the efforts of some of them to improve it—largely fulfills the claim made by Edwin O. Reischauer in his foreword that it "will always serve as a basic introduction to the story of a conscious women's movement in Japan" (p. xvii). Dorothy Robins-Mowry, who joined the U.S. Information Service there in 1963 and served for eight years, makes excellent use of materials collected in interviews with such notables as the late Ichikawa Fusae as well as with a host of other equally famous and lesser known women, including some Americans directly involved in implementing policies affecting women during the occupation period. She uses both English- and Japanese-language publications by and about figures in the women's movement.

Part 1 (Background: The Loom of History) de-

votes a rather cursory chapter to "The Way of Tradition" and a much longer one to "The Way of Modernization," which offers a very useful guide to events, personalities, and organizations of the period from 1868 to 1945. Part 2 (The Fabric of Contemporary Times) contains the meat of the book in four substantial chapters. Part 3 (Design for the Future), a mere six pages in length, is a highly and—in this reader's opinion—unduly optimistic reading of what is to come. The appendixes offer valuable information not readily available in any other single source, including a list of major women's organizations and the number of women in the national Diet from 1946 to 1980 as well as their party affiliations.

In view of the popular conceptions of woman's place in Japanese society, most foreign readers will be surprised to learn of the vigorously activist character of the women's movement in that country between the two world wars. The informed will not be surprised to learn of the persistent efforts of the authorities to suppress it. Many of the movement's major figures survived World War II to participate in the revival of the struggle during the occupation period when, the author correctly observes, Japanese women made unprecedented strides toward equality. Most of these redoubtable women, some of whom had received their education in the United States, have died or retired from active involvement within the last decade. Robins-Mowry rightly stresses the activities of the new generation of leaders and the new causes they champion. Those causes are many, for, despite the progress made by Japanese women in the postwar years, they continue to suffer cruel handicaps that will not be removed willingly by the male-dominated establishment.

The book is short on analysis, long on people, places, events, and documents. For anyone interested in obtaining a general orientation to a very complex matter, meticulously detailed, it would be impossible to suggest a better sourcebook. The bibliography alone comprises a kind of basic reading list in both languages on the perennially perplexing question: "where do women actually stand in Japanese society?" The extent to which the answer remains unclear is no fault of the author's but rather a reflection of the considerable ambiguities of the position of Japanese women today.

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DAVID C. COLE and YUNG CHUL PARK. *Financial Development in Korea, 1945–1978*. (Harvard East Asian Monographs, number 106; Studies in the Modernization of the Republic of Korea, 1945–1975.) Cambridge: Council on East Asian Studies, Harvard

University; distributed by Harvard University Press. 1983. Pp. xxii, 349. \$15.00.

The central question that underlies this study by David C. Cole and Yung Chul Park is the role of financial institutions in economic development. Does it make a measurable difference in the speed and character of economic growth how large a country's financial structure is compared to its national wealth and product? As such, the present study is best perceived in the light of the work done by Goldsmith, Gurley, Shaw, McKinnon, and others. In fact, to paraphrase Goldsmith, Cole and Park show once again that a good analysis of the role of the financial sector in economic development has to be profoundly historical.

It is not possible in a short review to follow the various strands of the Cole-Park exposé on the competition among the various models of financial development proposed for Korea during the last thirty years, nor to follow their thesis on the interactions between the export-led growth strategy, monetary policy, trade policy, sectoral policies, and financial policy on one hand and the structure of the financial sector on the other. Three elements of the study, however, deserve special attention: the functional role played by unregulated, as opposed to "unorganized" or "informal," markets in Korea; the outline of major financial periods in Korea; and, most significant for future policies, the long tradition of audacious policy moves in Korea.

Informal money markets and money lenders have a bad reputation in most developing countries and typically remain overlooked by analysts for lack of solid information. The dualistic structure of financial markets in Korea is better understood. Park, in various studies, has made a convincing case that in Korea the basic distinction is one of regulation not of organization. The unregulated financial market (UFM) has several components in Korea: a rudimentary private credit market that is generally what many development specialists have in mind when they speak of informal money markets; the *Kye* system (rotating credit associations) well studied in Korea in particular by Kwan-Chi Oh; the curb market; and private financial companies, or *mujin*. The dominant part of the UFM is a short-term market for primary securities maturing in one year or less where the ultimate borrowers and lenders are households and businessmen. The functional complementarity between bank loans and informal credit is important for interest rate determination on the UFM and the explanation of the ebb and flow of the UFM in response to financial policies.

Regarding the regulated institutional sector, Cole and Park identify a rough pattern of five-year time periods in postwar Korean history. During the war effort of 1950–55, the government controlled all

financial institutions. During the period 1955–60, commercial banks were shifted to the private sector under a strong central bank and experienced a relative decline compared to new special banks. Relative confusion reigned from 1960 to 1965, when the government not only asserted total control over the central bank, rationing a limited supply of credit to commercial banks, and went through an ill-conceived currency reform but also created new institutions, particularly local private banks. A minor golden age following the interest rate reform gave a new role to the banking sector and led to the financial take off during the period 1965–70, with total bank deposits rising from 10 to 30 percent of the GNP in five years. The period 1970–78 saw consolidation and relatively slower growth in the banking sector, a greater emphasis on nonbank financial institutions, and a frontal but unsuccessful attack in 1972 on the UFM. This was a period of innovation and financial deepening, new short- and long-term financial instruments, and greater institutional differentiation.

Cole and Park very effectively address the major current question of what the financial liberalization so strongly advocated by leading members of the government will actually amount to. One intriguing element of the answer is the long-standing Korean tradition of bold moves in financial policy, among which are King Kojong's prohibition of Chinese money in January 1872, the ill-fated currency reform of June 1962, the landmark monetary reform of September 1965, the Presidential Emergency Degree of August 1972, and the most unusual domestic interest cut of June 1982 in an environment of high world interest rates. Is the weight of history leaning against a hands-off government attitude toward financial institutions?

BERTRAND RENAUD
The World Bank

FRANK F. CONLON. *A Caste in a Changing World: The Chitrapur Saraswat Brahmins, 1700–1935*. Berkeley and Los Angeles: University of California Press, for the Center for South and Southeast Asia Studies, University of California, Berkeley. 1977. Pp. xv, 255. \$15.00.

This study by Frank F. Conlon might be categorized as belonging to the third generation of historiography on modernization in South Asia. In crude outline the first generation focused on the relationship between British rule and Indian nationalism; the second narrowed the scope to specific aspects of the British presence such as land revenue policies or Western education and the response of particular regions or religious communities; the third both constricted and broadened the view by concentrat-

ing on an individual social group or institution, as here on a caste of about 21,000, and by linking the British to their political predecessors. The third generation has also utilized new types of sources: Conlon's range from British government records to those of private institutions, including temples and caste associations, private papers, vernacular publications, and over fifty oral interviews.

In 1708 the Chitrapur Saraswat Brahmins signified their separate status by establishing both a new lineage of gurus, who were to be both religious preceptors and arbitrators of social behavior, and shortly afterward a *matha* or temple complex. Evolving from a cluster of Gaud Saraswat Brahmins who had migrated from north India on the Saraswati River to Goa on the southwestern coast of India, the Chitrapur Saraswats had a heritage of adaptation in physical mobility, education, and occupations that included both priestly functions and secular ones, mainly landholding and service professions where literacy was required.

Although he does not use many archival sources for this section, Conlon traces the Saraswat relations with both the Portuguese and the Marathas. The arrival of the British in 1799 brought political stability after four decades of intermittent warfare, a more legalized system of land revenue collection that reduced occasions for extralegal extractions, new areas of government service, and diverse forms of higher education. Initially, government service offset the reduction in landed income, and the growing prosperity of the Chitrapur Brahmins led to more material support for the *matha* and an effort to rationalize its management.

Modernization, however, introduced sources of tension as well as caste solidarity. British administrative considerations led to a breakup of the Kanara district, where Chitrapur Saraswats were dominant; the northern half was attached to Bombay and the southern to Madras. The pursuit of higher education and government jobs meant migration to those two colonial urban centers. By 1900 there was a growing concentration of Saraswats in cities, especially Bombay, and an emerging demand for social reforms such as an end to prohibitions on widow remarriage and sea voyages. Simultaneously, the guru tried to strengthen ties to the *matha* and to call caste members back to more orthodox behavior. When the guru responded to reform pleas by excommunicating some caste members who had gone to England, the urban Saraswats grew indifferent to the guru and *matha*.

By 1915 the possibility of caste fission appeared real because of differences between orthodox and reform-oriented and rural- and urban-based Saraswats. Caste members in Bombay had not established traditional forms of corporate identity such as a branch *matha* but instead had started associations

that addressed urban needs. They founded a credit society, the first cooperative housing society in India, and the Kanara Saraswat Association, which accepted anyone who had Saraswat ancestors. A visit to Bombay in 1927 by a new guru known for his humility and dignity sparked an effort at reintegration. It culminated in a general meeting in 1929 that ratified social reform resolutions and regularized community support for the *matha*. Conlon argues that the Bombay Saraswats were ready for a reconciliation as economic uncertainties and the migration of an older generation to Bombay stimulated a return to traditional religious values. Here Conlon stops his narrative and promises a second monograph on the post-1930 era. Although one can sympathize with him about the problems of dealing with recent history, it would have been helpful to have an epilogue tracing the Chitrapur Saraswats into independent India rather than one restating the general conclusions of the study.

Conlon's analysis focuses on the institutions that unified the caste. These included traditional ones such as the lineage of gurus, the *matha*, and caste councils as well as modern entities such as cooperatives and caste associations based on caste membership but not on orthodox ideas of hierarchy and purity. Thus, modernization had an ambivalent influence, creating new economic opportunities but also new stresses. Caste ties provided the basis for institutions that would assist in meeting the challenges created by modernization. Given the nature of his sources, Conlon generally answers the pertinent questions. There are, however, two areas in which one wishes for more information. First, why were the urban Saraswats so willing to seek reconciliation, and, second, what was the role of marriage and kinship networks in fostering economic advancement and caste revitalization? Although the Chitrapur Saraswats might be unusual because of their heritage of adaptability, Conlon has produced a basic study for anyone interested in the impact of modernization on the resiliency of caste groups in India.

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C. A. BAYLY. *Rulers, Townsmen, and Bazaars: North Indian Society in the Age of British Expansion, 1770–1870*. (Cambridge South Asian Studies, number 28.) New York: Cambridge University Press. 1983. Pp. xiii, 489. \$59.50.

With this immensely important volume, C. A. Bayly has given us a full-length study of a central, yet surprisingly little studied, era in the history of India: the transition from Mughal to British rule in the

northern plains. The conventional view of a "Dark Century" following the death of Aurangzeb in 1707, for some years increasingly called into question, is here decisively overturned, and the events of those years are set in a new and suggestive framework. At the same time, for Bayly, the early decades of British rule cease to mark out a decisive transformation. The critical breakpoint for the growth of the colonial order is not the conquest of 1801–03 but the depression of the 1830s, followed at some distance by the consolidation of a new urban system under crown rule from the 1860s. Throughout, it is not dichotomies but the continuities binding together later Mughal and early British India that command Bayly's interest.

At the heart of the volume is an account of what Bayly perceives as a new social order built, so early as the 1760s, on the rubble of the Mughal empire. As political power diffused downward in the wake of Mughal collapse there emerged not only the well-known successor states (Awadh, Bengal) but also a host of intermediary powerholders. The large landholding *taluqdars* and *zamindars* have been the subject of much study. Bayly prefers to concentrate on what he calls the "service gentry," largely Muslim, of the small towns (*qasbas*) and the Hindu commercial groups. Together, he argues, these groups, with the regimes that nourished them, cushioned the end of centralized empire. Above all they provided the skills and patronage that made possible renewed growth in both agriculture and commerce. Although Bayly does not deny that some absolute decline took place, especially in the western areas near Delhi, he nevertheless insists that much apparent decline involved simply the redistribution of trade and production in the wake of shifts in demand that followed the relocation of political power. Two centers in particular stand out as nodal points of growth—Benares and Mirzapur—and to their study Bayly brings the rich documentation of early British local archives.

In the second half of the volume Bayly examines how the "new pattern of stability" created by 1800 shaped the subsequent character of British colonialism in the region. Initially, these "existing processes of change" buoyed up British commerce and government, triggering what Bayly calls "the last stage of the expansion of the precolonial political economy" (p. 225). In the 1830s and 1840s, however, laissez-faire politics and a trade depression together set in motion a "general crisis" of the Indian political system, of which the 1857 rebellion was only the most dramatic manifestation. Eventually, new institutions and new urban centers (above all the rising industrial city of Kanpur) brought renewed growth, but the many of the old intermediate groups, above all the literate gentry in declining small towns, had lost out irretrievably. The colonial crisis was at the

same time, Bayly argues, moral as well as economic, for it involved the disruption of the ties of patronage and status that had previously linked the localities to their rulers and had thereby legitimated rule. As a consequence, both the Hindu merchants and Muslim gentry sought security in independent organization. The stage was set for the communal rivalry and hostility of the twentieth century.

In part because of the range of Bayly's analysis, in part because of the paucity of sources, especially for the eighteenth century, much in the volume can be no more than suggestive. One of Bayly's central arguments is that during the post-Mughal years the Hindu merchant communities developed autonomous corporations focused around shared conceptions of religion and credit that cut across caste boundaries. In two extended chapters, Bayly describes the distinctive styles of life and business of North India's merchant families. The analysis is remarkable for its balancing of cultural and of economic motives—this is not a simplistic Marxist account; there is, however, scant evidence (apart from such exceptional groups as the Naupati Sabha of Benares) for the rise of those "corporate solidarities" that Bayly sees as bringing with them the "creation of a unified merchant class" (p. 463). In the process, Bayly may exaggerate, especially in light of the accounts by such scholars as K. Gillion and M. Pearson of merchant organizations in Gujarat, the fluid and court-centered structure of Mughal society. To pose a sharp break after 1707 may be as unwise as to do so at 1801. Similarly, Bayly's merchant class is at times oddly paired with the service gentry. Although both may have flourished in the wake of Mughal decline, and both gave vitality to urban life, their subsequent histories under colonial rule diverged markedly. In the end the merchants, unlike the small town gentry, successfully adapted to the new urban system of the later nineteenth century. To tie together, as Bayly does in his stimulating final chapter, "corporations, *qasbahs*, and the new politics," involves an imaginative leap that only future research could substantiate.

This volume is long, perhaps excessively so, and it requires close and sustained attention. On that account it is little likely to attract the nonspecialist reader. This is unfortunate, for students of economic development, of the Hindu religion, and of the structure and working of colonialism will all find much to stimulate their interest. Historians of India on their part will find a host of new insights brought together within a challenging, and almost always convincing, analytic framework. No one who seeks to understand the roots of modern India can afford to ignore this book.

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DAVID PAGE. *Prelude to Partition: The Indian Muslims and the Imperial System of Control, 1920–1932*. New York: Oxford University Press. 1982. Pp. xiii, 288. \$24.95.

Although originally written in 1974 as a doctoral dissertation at Oxford, David Page's *Prelude to Partition* is a full-fledged specimen of the so-called Cambridge school of modern Indian history. (See H. Spodeck, "Pluralist Politics in British India," *AHR* 84 [1979]: 688–707.) Page's supervisor, the late John Gallagher, who moved back and forth between the two universities, presided over an articulate and enterprising group of first generation postimperial British scholars in an attempt to come to terms with the loss of Britain's brightest jewel. It was the ambition of the Cambridge school to identify and document "The Structure of Politics" of the British Raj (as in the title of Page's first chapter) during the era conventionally identified with the Indian Nationalist Movement, from the 1880s to 1947, in much the same way that Lewis Namier and his followers had developed their interpretation of late eighteenth-century Britain. Why Indians entered politics was to be construed as a matter of local and particular self-serving interests, not ideology and fine speeches; but the petty details of "ins" and "outs" nonetheless were the stuff of constitutional transformations.

Namier, however, took the trouble to learn English before embarking on his great project. The Cambridge school, by contrast, had no use for any Indian language and considered Indian culture relevant to political history only insofar as it percolated into the discourse of official British documents and the private (English) papers of a small circle of Indian public figures. In explaining the historical basis of the partition of India in 1947—"one of the most significant events of modern history" (p. 1)—Page deliberately eschews any discussion of the meaning of categories like "Hindu" and "Muslim" and their relevance to social solidarity. Instead, he introduces a carefully selected cast of characters: British officials striving to maintain their "system of control" and scheming Indian politicians in search of office and wealth. Part of the calculation of these individuals was how to shore up their claims by developing "constituencies," and for some unexplained reason religious labels seem to have been remarkably effective in claiming the support, off-stage, of vaguely defined populations.

The reason such public support became part of the system was the gradual introduction, starting in the 1880s, of electoral institutions, which were placed alongside the political dominance of the British district officer—heretofore the "mother and father," as Page reminds us unsmilingly, of the Indian people. Designed to localize and contain

grievances, these innovations, first at the district and municipal and, after 1909, at the provincial level, retained in British hands the real stuff of politics, namely patronage. "But," writes Page, "the Raj did not stand firm" (p. 5), and, as a result, political competition after 1920 had to play itself out in ever-widening arenas. It is the heart of Page's argument that "the mainspring of nationalist politics remained competition of interest at the provincial level" (p. 6). The Montagu-Chelmsford Reforms cunningly drew in Indians willing to "work" the system and left all-India politicians, such as Gandhi and Jinnah, at the margins.

The groundwork for the partition of India, according to Page, can be discovered in the constitutional negotiations of the 1920s and 1930s, which created a large measure of provincial autonomy for those areas that were to become Pakistan. Most accounts of Muslim separatist politics dwell on all-India concerns or on those regions, particularly the United Provinces, in which Muslims were a relatively small minority. But the areas that became Pakistan were those in which Muslims were a majority, and the desire of Muslim politicians to reap the benefit of that majority kept them from allowing the compromises that U.P. and other minority province Muslims wanted. It was not Muhammad Ali Jinnah who laid the foundation of Pakistan, for as an all-India politician from Bombay he continually sought accommodation with the Congress. The major Muslim figure in Page's account is Sir Fazl-i Husai (1877–1936)—to whom Page refers ungrammatically and condescendingly as "Fazli"—leader of the professedly noncommunal party of Punjabi landowners, the Unionists. With Indian self-rule vested in the provinces but withheld at the center, a politician like Jinnah had nowhere to go: only then did he formulate the concept of "Muslim" as the basis of a national identity.

What Herbert Butterfield said of George III can also stand for Muhammad Ali Jinnah: "even" he had principles. One can be grateful to Page for detailed accounts of constitutional politics and his salutary emphasis on the pivotal importance of the Punjab, but a history of Indian politics finally does not work without some attempt to understand indigenous Indian perspectives. There is a great gap between 1932 and the creation of Pakistan in 1947, and no one will understand it without a healthy respect for the power of symbols.

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JAMES MANOR. *Political Change in an Indian State: Mysore, 1917–1955*. (Australian National University Monographs on South Asia, number 2.) Columbia, Mo.: South Asia Books. 1978. Pp. xii, 261.

James Manor's study of political change in Mysore State prior to Indian independence in 1947 is, in a number of ways, admirable and well worth serious reading. The author bases his volume on thorough research in relevant records and has put us in his debt by his skillful use of oral history to reinforce written evidence. Manor's grasp of politics and the political system in the Indian princely state of Mysore after World War I is comprehensive and effective. The account is also enriched by careful attention to changes in the larger society and economy that were relevant to the political system in the state. In all, Manor provides an excellent, informed picture of political dynamics in Mysore between 1917 and 1947.

His handling of the complexities of caste in Mysore is less felicitous and his grasp of the subject somewhat less than wholly effective. Nonetheless, Manor's treatment of caste in Mysore is adequate. Similarly, his discussion of the land tenure system could have been more penetrating. These limitations do not detract seriously from the very good study of political change and of important political figures. Chapter 9, on politics after independence, is relatively superficial and does not display the level of informed understanding that characterized the pre-1947 material. Overall, the presentation is clear and easy to follow, even though the style is by no means elegant or vivid.

The study, however, is diminished in value by certain features that need mention. Manor's obsession with the role of "patronage" and "political spoils" is daunting and is repeated so often as to become tedious. Patronage and spoils have been crucial to politics at least since Sir Robert Walpole's era and were the building blocks of Tammany Hall in the United States. They do not explain any political system.

Manor concentrates on the linkages among local, state, and national politics in India. Anyone familiar with politics in the United States, however, would be fully at home with these linkages, the transactions and consequences of which are detailed so carefully by Manor. Likewise, his synoptic account of political modernization in Mysore after 1947 reads like a textbook on the subject applicable to any geographic region. Generally, he tends to see the details rather than the general picture, and that is detrimental to the overall value of the book. *Inter alia*, this may be why he underestimates or discounts the role and the importance of all-India nationalism and the significance of ideas or values in political behavior, vis-à-vis Mysore. For politics inside Mysore, 1917–47, the book is excellent, providing substantial information and good exposition.

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RAJAT K. RAY. *Industrialization in India: Growth and Conflict in the Private Corporate Sector, 1914–47*. New York: Oxford University Press. 1979. Pp. xi, 384. \$15.95.

Rajat K. Ray's book is particularly useful in its discussion of India's early industrialists and their relationship to the British Raj, the Congress, and each other. It concludes, correctly, that the policies of the British government were inconsistent during 1914–47. Although the government became more supportive of Indian industrialization after World War I, encouragement was given to some industries but not to others. Finally, Ray argues that Indian businessmen were driven by the profit motive and were willing to assume greater risks and to plow more earnings back into their companies than their British counterparts. For this reason, by 1947 Indian-owned companies supplanted British-owned companies in many industries.

Ray's analysis of these issues occupies about 80 percent of the book. It is generally well done and contains almost all of Ray's original research. What is troublesome is the remaining 20 percent. This, unfortunately, consists primarily of categorical expressions of faith that are not supported by scholarly analysis and do not always bear directly on the principal subject of the book—the industrialization of India between 1914 and 1947.

Most disturbing is Ray's frequent and often florid assertion that pre-independence India needed a "massive and formidable government programme of intervention in the economy" (pp. 229–30), that "there could be no soft approach, no line of least resistance," and that "only savage, single-minded determination and will-power, that brooked no obstacle or resistance, that did not quail at any sight of privation and suffering, could have carried the Indian economy forward at a pace comparable to that of Japan or Russia during the inter-war period" (p. 234). "The solution," according to Ray, "was large-scale government intervention in the economy and a planned approach to economic growth" (p. 340).

Even so, between 1913 and 1938 India's industrial output grew at a compound annual rate of 3.6 percent, compared to 2.4 percent for the world as a whole. India ranked sixth of twenty-eight countries listed by Ray. Heading this list are South Africa (9.9 percent), the USSR (9.0 percent), and Japan (7.1 percent). Even more impressive, India's annual rate of industrial growth increased steadily during the 25-year period, rising from 2.4 percent between 1913 and 1920, to 3.2 percent between 1920 and 1929, and to 4.8 percent between 1929 and 1938 (computed from data on page 17).

But this was not enough. According to Ray, India's industrial growth could have been higher had the preindependence government, like the postindependence government, made the massive public investment that was necessary (p. 257). There are several flaws in this argument. Most importantly, rupees invested do not measure output, but only the cost of one input—newly installed capital. Investment in idle plants or unwanted inventories does not benefit the Indian nation. If anything, pre-independence India is to be commended for achieving so high a rate of industrial growth at so low a level of public investment.

Ray compares India's performance unfavorably with that of Japan and Russia, asserting that the difference lies in lack of planning under the British Raj (pp. 230, 234, and 350). He does not mention the one country that posted the best performance of all, South Africa. He also largely ignores the fact that Japan did not adopt comprehensive planning but relied instead on private initiative with substantial support by the government. Nor does he mention that the USSR adopted planning only in 1929 and that there was fairly rapid industrial growth under the tsars.

Nor does Ray discuss India's postindependence performance under planning, the model that, he believes, pre-independence India should have followed (p. 365). India's annual rate of industrial growth rose to about 7.0 percent after 1951. This increase, however, was not commensurate with the greater resources that India's planners devoted to industrialization. At the same time, India's rate of economic growth, as measured by real GDP, has remained essentially unchanged since the early 1950s. India's per capita income has increased, but by only 1.5 percent per year.

This points up what is, perhaps, the most serious flaw in Ray's argument. Industrial growth is not the same as economic growth. Developed economies have low rates of population growth and efficient agricultural sectors as well as modern industrial sectors. To many observers, the greatest weakness of Indian planning was its failure to recognize this fact until very late.

Ray also discusses at some length the reasons why village India failed to develop during the Mughul and British periods (pp. 344–52). His argument, an afterthought presented without documentation in the conclusion to the book, is that the surplus generated by village India was extracted by "parasitic" governments, presumably through some form of compulsion (p. 345). Ray is critical of this. Yet, it is exactly what he would have done under comprehensive planning during the pre-independence period to achieve a higher rate of industrial growth. Given India's performance under planning during

the last three decades, it is by no means clear that the sacrifice by village India can be justified by the slightly higher rate of industrial growth achieved.

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MARY FAINSDOD KATZENSTEIN. *Ethnicity and Equality: The Shiv Sena Party and Preferential Policies in Bombay*. Ithaca: Cornell University Press. 1979. Pp. 237. \$19.50.

It is a pleasure to read a book that lives up to the author's preface. Mary Fainsod Katzenstein explores the relationship between the facts of ethnicity (defined in terms of group identity and group interests) and the norms of equality (defined as equality of opportunity and recognition of individual merit) in India's most modern city, Bombay. She did her field research and some writing in the early 1970s. At that time the sudden emergence of a powerful ethnic party, the Shiv Sena, had aroused strong feelings. Ten years later controversy remains, but the force of ethnicity in Indian politics is much more in evidence, as it is in other democratic polities as well. Therefore Katzenstein's findings acquire increased salience.

Ethnicity and Equality is limited to a study of Maharashtrians in Bombay city and does not deal with the general question of giving or withholding preferential treatment to groups and thereby affecting their economic and political competitiveness in an open system. Despite its topicality, the book makes an important contribution to the study of contemporary political systems. Ethnicity, says the author, is a distinctly modern phenomenon. It is the product of national integration and social mobility in a country where government has an important role in allocating economic benefits. Demographic factors of high migration into the city and low migration out of it do not alone explain the rise of the Shiv Sena in Bombay. The decisive fact was competition for middle-class office jobs between the "native" Maharashtrians and the more qualified South Indians. According to this analysis, then, ethnicity is different from India's traditional kinship identity where occupations were ascriptive and non-competitive in a stratified society. Ethnicity, or nativism, is also different from romantic nationalism that seeks a territorial embodiment for sentiments of separate identity. If Katzenstein can be believed, the upsurge of ethnic or "sons of the soil" movements in different parts of India is *not* symptomatic of disintegration into primordial chaos but, rather, evidence of robust and open politics.

Katzenstein explains the origins, emergence, and success of the Shiv Sena after 1965–66 in a systemat-

ic series of chapters, easy to read and well documented with tables, charts, and reports of interviews. She highlights the lower-middle-class character of the party—Maharashtrians are not yet competing in big business—and its youthful membership. She shows the crucial roles played by the newspaper *Marmik*, the tight discipline wielded by the founder Bal Thackeray, the extra quick communications system established throughout the city, the extremist rhetoric, and the willingness to use violent mob action to build up Shiv Sena's power. The judicial system was also caught in the dilemma of reconciling individual merit with group preferences. The courts developed no explicit doctrine but made case-by-case rulings that leaned toward localism.

The author deliberately refrains from normative assessments. She refuses to explore the larger implications of what she reports: the limited demands of the Shiv Sena and its lack of challenge to the existing educational or political systems; official patronage for many of the Shiv Sena's demands; the diminishing strength of the Shiv Sena after 1975. Therefore, Katzenstein's final chapter, "Modernization and Meritocracy," is a somewhat disappointing recapitulation of the book. Dipankar Gupta's *Nativism in a Metropolis* (1982) goes further, with the help of Marxist analysis.

Finally, one is left with questions about Cornell's publishing schedule. In some chapters events of the late 1960s and early 1970s are discussed in the present tense, but the book did not appear until 1979. Nevertheless, it remains worth reading in the 1980s because its careful study of the particular can help shed light on the general. And the general subject of ethnicity, equality, and modernity is certainly not confined to Bombay or even to India; it is of world-wide concern.

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HUE-TAM HO TAI. *Millenarianism and Peasant Politics in Vietnam*. (Harvard East Asian Series, number 99.) Cambridge: Harvard University Press. 1983. Pp. xii, 220. \$30.00.

Rural religious movements have long fascinated writers in Asia as well as the West. They provide a window whereby intellectuals, usually of urban elite backgrounds, can peer into a strange world of illiterate farmers, peddlers, itinerate priests, and medicine men. Moreover, when conditions are ripe, such movements can suddenly take on more than local significance, picking up hundreds of thousands of followers and posing dilemmas for both established religion and the state. Although the odds are heavily stacked against such village-based

groups achieving nationwide success, they often leave more than temporary historical imprints.

In this compact, well-written book, Hue-Tam Ho Tai focuses her attention primarily on Buddhist millenarian attitudes and behavior in the western Mekong delta region of southern Vietnam between 1850 and 1950. She is well qualified to tackle this difficult topic, not only in terms of disciplinary training and language skills, but because her family came from that region originally and she grew up amid a host of relevant tales, poems, and reminiscences. That is no small advantage when one considers how few written records were left behind. Ho Tai, however, is neither a millenarian believer herself nor old enough to have participated in the events discussed, which gives her account objectivity.

The Mekong delta of the mid-nineteenth century was a rough-and-ready frontier society, not unlike the American West of the same period. Political authority was tenuous, social relations unstable, and religious currents diverse. Theravada and Mahayana Buddhism, Taoism, Confucianism, and assorted spirit cults influenced the population in varying degrees. Ethnic Vietnamese interacted with Cambodians and Chinese, and all were forced to accept French colonial rule from 1867.

Out of this complex brew emerged the Buu Son Ky Huong (Strange Fragrance from the Precious Mountain) movement, the prime object of Ho Tai's attention. Scholars of comparative religion will particularly appreciate the author's discussion of millenarian linkages between self-perfection and action, between apocalyptic prophecy and worldly involvement. Historians of Vietnam will learn a great deal about rural Cochinchina before World War I.

This book, however, ought to provoke the most interest among sociologists and political scientists who study rural peoples undergoing rapid change. Although she does not spell it out, Ho Tai writes within the context of an ongoing scholarly polemic between "moral economists" and "political economists." Readers of either inclination will find ammunition to bolster their position, a fact highlighted by the publisher's success in obtaining laudatory quotations from the standard bearers of both sides to place on the dust jacket. Uncommitted readers will wonder if the evidence in effect calls into question both theoretical constructs.

I am not convinced by Ho Tai's argument that the Hoa Hao sect, which grew out of the Buu Son Ky Huong tradition from 1939, represented an effective competing ideology of change to that offered Vietnamese peasants by the Indochinese Communist party. Unless one is using the term in a perjorative sense, "ideology" cannot be totally subjective. It must enable adherents to deal with reality and to learn from experience. Ho Tai admits that failure bred failure in Mekong delta millenarianism. Be-

cause each unsuccessful rising reinforced visions of ultimate doom, it merely served as dress rehearsal for the next outburst.

Because Hoa Hao recruitment depended on the public exercise of magic and prophecy, secrecy against enemy attack often proved impossible. Even when secrecy was sustained, too much depended on a single charismatic leader. After his violent death in 1947 a reincarnation was never found, causing the group to lose momentum and then to slide toward factionalism, parochialism, and manipulation by outside organizations. More fundamentally, however, the Hoa Hao, with its antforeign, antiurban, and antirational bias, could not hope to meet the challenges faced by the mid-twentieth-century Vietnamese.

Needless to say, communist parties have their own problems with reality, yet they pale in comparison with those of millenarian groups like the Hoa Hao. In the last sentences of the book, Ho Tai suggests that the millenarian quest for a perfect community through all-consuming violence is not dead in Vietnam. That may well be true, but it unfortunately brings believers no closer to salvation in this world than it did forty or one hundred years ago.

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IAN BLACK. *A Gambling Style of Government: The Establishment of the Chartered Company's Rule in Sabah, 1878-1915.* (East Asian Historical Monographs.) New York: Oxford University Press. 1983. Pp. vi, 254. \$26.00.

Toward the end of the nineteenth century the Colonial Office considered alternative ways to secure tropical frontier regions without the full support implicit in the extension of British sovereignty. New experimental techniques of "informal empire" were introduced that included the residency system, extraterritorial jurisdiction, the protectorate system, and chartered companies. Of the various alternative experiments adopted in subsequent decades by exponents of informal empire, perhaps the most interesting was the chartered company. Regarded skeptically as visionary, speculative, and susceptible to corruption, this mode of government was revived in North Borneo, in the Malaysian state of Sabah, in what has been referred to as an imperial fit of absent-mindedness.

The main story of Ian Black's book is the incredible account of how a private chartered company that was underfinanced, understaffed, and ill-equipped and trained in colonial administration managed to govern a vast territory fragmented politically, ethnically, and culturally. This tale, with all the dash and spice of a keystone cops drama, is

not well known. Black is the first person who has tried to create a history of Sabah under the company or to reinterpret the faltering process of colonial occupation and administration that has euphemistically passed for company rule and "history." On the whole, he is successful in telling the story of speculative businessmen dabbling in colonial real estate, of a ramshackle administration operating on a shoestring budget, of a polyglot police force that was understaffed and ill-disciplined, and of Sabahans who came to be classified as rebels or as a "martial" people, a familiar note in the annals of colonial warfare. It was indeed "A Gambling Style of Government."

Most interesting, perhaps, is the emphasis Black gives to conflicts that broke out as the chartered company struggled to extend its regime over a territory as large and diverse as North Borneo. He details the career and popular resistance to the company of the Muslim "rebel" Mat Salleh, and the tragic revolt of the southern Murut people in 1915. Black also rightly notes that as the company struggled to draw territory under its control its officers had to adapt administrative technique to suit the company's feeble financial underpinnings. They used concepts inherent in the various traditional systems to introduce social and political change without undue expenditure or extensive bureaucracy. The company was able, for example, to mobilize neighboring warlike Iban to dampen resistance to its overlordship.

I want to comment on the kinds of historical sources used in preparing this study. More could have been made of the distinctly rural character of the "other side of the frontier." Tragically, very few records at the district level so vital to a history of the people of Sabah have survived. Black, who first visited Sabah as early as 1966, has competently used a wide range of original sources, including the company's central government files. It is a pity that he did not rely to some extent on oral history. In his research to create a history of Sabah under the company, the author might have been surprised and impressed with how much he could have done with the recollections of old rebels, swidden agriculturalists, Chinese pioneers, and their descendants. This is a single criticism, however. The book is an important contribution to the historical literature on Malaysia. It is well written and provided with suitable maps and plates.

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LEONARD Y. ANDAYA. *The Heritage of Arung Palakka: A History of South Sulawesi (Celebes) in the Seventeenth Century*. (Verhandelingen van het Koninklijk Instituut voor Taal-, Land- en Volkenkunde, number

91.) The Hague: Martinus Nijhoff. 1981. Pp. ix, 353. f. 110.

This is the first major history of the South Sulawesi area of Indonesia in the seventeenth century in English to have appeared from the pen of a historian who has a mastery of both Dutch- and Bugis- (South Sulawesi) language sources. Leonard Y. Andaya, previously known for his painstaking study of the Malay kingdom of Johor in the seventeenth and early eighteenth centuries, has managed to present here a richly detailed account of the political evolution of the South Sulawesi area during the latter part of the seventeenth century, when it achieved a certain unity under the aegis of the remarkable Bugis leader, Arung Palakka La Tenritatta (ca. 1635–96). Taking as his main theme the life and achievements of this noted warrior, Andaya has given an account of state and society in South Sulawesi during this period, and he describes the background to the conflict between the powerful kingdom of Goa and the Dutch East India Company. He also discusses the Makassar War (1666–67), which led to the humbling of Goa and the return of Arung Palakka to South Sulawesi as the foremost ally of the company. The Treaty of Bungaya (November 18, 1667) temporarily ended the war and served as a basis for a new political order in the region. The subsequent campaigns waged by Palakka and the Dutch brought Goa and its allies finally to heel. Joint political overlordship was established by the former after the fall of the last Goa stronghold of Somboapu on June 24, 1669. The joint overlordship encountered problems, however, in the 1670s. Difficulties were caused by the refugees from Makassar in Java and their eventual defeat by Palakka and the Dutch in 1679. Palakka's own authority within South Sulawesi was challenged after his return from Java. Palakka had to take astute measures to secure the succession of his nephew, La Patau, and he fashioned a strong structure of marriage alliances and unchallenged political dominance after his successful conclusion of the Toraja War of 1683. Palakka's political legacy is also briefly considered. Two appendixes give the details of the rulers of the major kingdoms in South Sulawesi in the seventeenth century. An "abridged" English translation of the Bungaya treaty as well as a glossary of local terms, twelve pages of notes amplifying some of the detailed points made in the chapters, a bibliography, an expertly compiled index, and nine maps are included.

This all adds up to a work of the sort of impeccable scholarship one has been led now to expect from Andaya, and it is hard (for this reviewer at least) to fault him on points of detail. Yet at the end one is left wishing that much greater space had been devoted to social and economic affairs. Occasional,

tantalizing glimpses are given of crucial agricultural, commercial, and demographic developments, but, on the whole, these are firmly subordinated to the political narrative. Even an issue as important as the slave trade of the eastern archipelago, which was long dominated by the Makassarese, does not rate a mention. Perhaps it had no bearing on the story that Andaya has to tell, but it seems an important omission nevertheless. Despite Andaya's mastery of the local sources and the extensive interviews he has conducted with a wide range of people in South Sulawesi, the history he has presented is a distinctly conventional one: high politics, marriage diplomacy, military campaigns, flamboyant personalities, and *faits divers* dominate. These are the stuff of the Dutch reports and the South Sulawesi *lontara* (chronicles) and they hold Andaya in their sway. Even his slightly weighty prose style seems to reflect their influence. His achievement, however, remains a solid and valuable one. The history of Arung Palakka has much to tell us about the way the Dutch East India Company managed its affairs in the seventeenth century and the nature of its dependence on local Indonesian rulers. As such, Andaya's book will be of interest to Indonesian specialists and company historians alike.

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UNITED STATES

ROBERT M. WEIR. *Colonial South Carolina: A History*. (History of the American Colonies.) Millwood, N.Y.: KTO Press. 1983. Pp. xix, 409. \$30.00.

Robert M. Weir sees several distinctive themes in the development of early South Carolina: the growth of a black majority; the effect of its presence on the whites; the development of an economy sufficiently prosperous to maintain some South Carolinians in exceptional wealth; and the growth of an increasingly stable and autonomous polity, one threatened during the 1760s by the intervention of British authorities and a massive influx of new settlers into the back country. While New Yorkers seemed factious and Pennsylvanians licentious in their politics, South Carolinians—at least in the eighteenth century—eschewed such strife. During the first two generations of settlement divisions were sharp, but following the overthrow of the proprietary regime the royal administration did not, as elsewhere, use appointments to the council to reward local leaders, to create a political faction, but rather to endow outside placemen. Hence the provincial elite, although attracted to English culture, concentrated in

the Commons House of Assembly where it sought to build up its power using ideas and rhetoric common to opposition politicians on both sides of the Atlantic.

Even more striking, by 1708 half of the colony's population was black; by 1720, two-thirds, and in some parishes in the low country slaves outnumbered whites by seven or eight to one. Why did planters rely so heavily on black slaves rather than other labor? The unhealthy swamps of the low country and the resulting high mortality rate may have made the region unattractive for white labor. Only in a bibliographical note does Weir point to the relative importance of naval stores and rice in initially stimulating the demand for slaves. But as was illustrated in the forests of New England and on the Chesapeake, it was possible to produce naval stores and staples for export with labor other than black slaves, with white families and indentured servants. The answer may lie in the influence of the earliest leaders in South Carolina, planters from the West Indies already familiar with a plantation economy and slave labor, men seeking immediate profits and not concerned with the long-term consequences for Carolina society. Many of these planters as well as merchants who came out from England did not, initially at least, plan to remain in the colony for an extended period, but it took more than a generation for the leading families to acquire sufficient wealth. By the time they could retire to England, many no longer wished to do so.

Until the middle third of the eighteenth century there was little increase among the whites; at 1750 the white population stood at roughly twenty-five thousand. During the next twenty years it doubled and then experienced a threefold increase by 1789, the result of heavy emigration from the North and Europe. By the close of the war with France the back country contained about half of the white population; by 1770 at least two-thirds and by 1790 nearly four-fifths of the one hundred and forty thousand whites lived outside the low country.

But the opening of the back country to settlement, the ensuing war with the Cherokee, and the influx of thousands of white families strained the political fabric of the colony as the provincial elite, ensconced in the lower house and unwilling to surrender control in the assembly, failed to respond quickly and fully enough to the needs of the newer communities. Nor did the South Carolina Whigs initiate action with the onset of tension with the mother country; they followed the examples set first in other colonies. The Charles Town elite was upset by British "hauteur," found English arrogance frightening, and saw in the failure of imperial authorities to recognize its claims and pretension, a threat "to emasculate them on their own turf" (p. 286). Yet Weir enters a disclaimer. Local leaders

did not eventually espouse revolution solely—or even primarily—to preserve their own position. What rankled with them was the unfairness of the British view that they had not done enough against the French and the Cherokee. But where in all of this is British “tyranny” or “slavery”?

The critical months came with the outbreak of the Revolution in the North as the Whigs of the low country conducted a campaign of persuasion and coercion to divide the back-country groups, to intimidate neutrals, and to conquer the militant loyalists. The Revolutionary regime made some concessions to the back-country settlers, but not until 1808 did voters in the west, a majority among the whites, elect the majority of the representatives in the legislature. But by then the distinction between coast and interior was blurred as, one suspects, the heavy reliance on slave labor permeated the state.

A further point remains: the source of the militant parochialism that expressed itself in the antebellum period. Was the Carolina “fire eater” the legitimate heir of the colonial political tradition, as Weir suggests, or was he the spokesman of a white minority determined not to allow outsiders, either in London or Washington, to challenge established racial relations?

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IRA BERLIN and RONALD HOFFMAN, editors. *Slavery and Freedom in the Age of the American Revolution*. (Perspectives on the American Revolution.) Charlottesville: University Press of Virginia, for the United States Capitol Historical Society. 1983. Pp. xxvii, 314. \$15.95.

The period between the Revolutionary War and the second decade of the nineteenth century saw dramatic changes in the nature of slavery within the United States (as well as elsewhere in the Americas), not all of which were linked directly to the Revolution and its impact. Among the major changes in this period were the ending of slavery and the growth in the number of free blacks in the northern states; a dramatic upsurge in the number of manumissions, particularly in Virginia and Maryland; and, in addition to the drift westward within the settled states, the beginnings of the dramatic movement of slaves to the west and to the south. As a result of these changes, in terms of geography and crops produced, the last half-century of slavery in the United States was quite different from its first century-and-a-half.

This set of ten papers, first presented at a conference of the United States Capitol Historical Society, deals with various aspects of the changing pattern of

American slavery in “the Age of Revolution.” They are indicative of the present state-of-the-art in slave studies regarding questions, data, and interpretation, and serve as a useful survey of slavery in a period for which major studies remain to be done. The book is divided into four sections, the longest of which includes four essays, each dealing with a different geographic area. Gary B. Nash examines detailed data on occupation, family patterns, and the black community in northern cities, using a variety of sources—censuses, tax lists, and probate inventories. He discusses northern urban slavery, going back a century before the Revolution, as well as the emergence of a free black community. Richard S. Dunn focuses on the changing numbers and location of slaves and free blacks in the Chesapeake from 1775 to 1810, describing the continued expansion of slavery in Virginia and its relative decline in Maryland. In the longest essay, and the one presenting the most new data, Philip D. Morgan analyzes changes in the South Carolina low country. Through detailed discussions of demographic, economic, and cultural changes, Morgan argues that the Revolutionary era saw “the growing entrenchment of slavery and the growing autonomy of the black community” (p. 83). Allan Kulikoff’s essay is an introductory foray into the estimation and description of the onset of the large-scale westward movement of the slave population. It raises important questions about the origins and geographic transferal of Afro-American culture and society.

There are two essays on major institutions in slave society, one by Mary Beth Norton, Herbert G. Gutman, and Ira Berlin on the slave family, the other by Albert J. Raboteau on the slave church. Both of these supplement earlier writings by the authors on these topics. Duncan J. MacLeod (in the only one of the essays previously published) summarizes the changing attitudes of whites toward blacks and of blacks toward whites, in the two centuries before 1820, and he suggests “a new periodization for the study of slavery in the colonial era” (p. 217). In his detailed description of the impact of the Revolution on Caribbean slavery, Franklin W. Knight traces links to the Haitian Revolution as well as to an ultimate weakening of slavery in the British empire. The latter point is also made by David Brion Davis, in his “disciplined fantasy” imagining a world with the colonies and their slaves still a part of the British empire—a world in which, he argues, it is doubtful that the legislated British emancipation in 1833 could have taken place. The final essay, by Benjamin Quarles, examines the impact of the Revolution on black society and the black quest for freedom in southern and northern states.

The quality of the essays is high, and much new information is provided. Reflecting the present historiography, they point to several major tensions in

writing about slavery. Determining the nature of master-slave interaction and the influences on slave and master belief and behavior remain difficult issues. There are also problems in achieving a balance between an emphasis placed on the unity of black societies and their diversities (in what Ira Berlin in the introduction calls the "three slave systems" of mainland North America [p. xvi]), as well as that between an attention to cultural changes over time and a focus on constants over long periods and in different locations. Thus these essays are necessary reading, both for the new material presented for the period from the Revolutionary War to 1820 and as an indication of recent developments in the study of slavery in the United States.

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JANICE POTTER. *The Liberty We Seek: Loyalist Ideology in Colonial New York and Massachusetts*. Cambridge: Harvard University Press. 1983. Pp. x, 238. \$22.50.

The American Revolution was an incomprehensible event to the loyalists. They could not understand why their colonial brethren, a relatively free and prosperous people, would revolt against such beneficent governments as existed in America and in London.

Historians—American historians, that is—have been almost as mystified by the behavior of the loyalists. Why did they not join in the protest against arbitrary rule? Why did they refuse to support a rebellion for republicanism and independence? During the past twenty years a spate of scholarly works treating loyalist activities has appeared, reviving an interest in these people that had inexplicably waned for a generation or two. Most of the earliest recent studies probed such matters as Tory demographics and loyalists in exile. These monographs were followed by biographical treatments of the better known loyalists. In the past half-dozen years biographies of Joseph Galloway, Thomas Hutchinson, Jonathan Sewall, and Jonathan Boucher, among others, have appeared; many of these works have posited on the ideology of the Tory under scrutiny.

One might then ask whether still another study of the loyalists, one that also examines the philosophy of these unfortunate people, is required. Janice Potter believes it was needed, and we are richer for her endeavor. She has produced a thoughtful book that summarizes and analyses the thought of articulate Tories from New York and Massachusetts.

Some chapters of Potter's study do not offer much that is new. She reviews loyalist ideas and chronicles the media through which they promulgated their beliefs. In addition she identifies the

chief Tory propagandists and ideologues. Specialists will not discover much that is new in these sections, but generalists will find these chapters to be a splendid summary, perhaps the best yet published, of the full panoply of Tory ideology.

Having laid the groundwork, Potter—especially in her middle chapters—moves into less well explored terrain. Here she explores the sources of Tory rhetoric, and she produces some surprises. Many of the loyalists' warnings about actions and attitudes that could lead to tyranny and chaos, she finds, were familiar to colonial readers. Admonitions about factionalism and democratic impulses were commonplace in political disputes that erupted in New York and Massachusetts long before the movement for independence occurred, Potter argues; moreover, many of the authors of those cautionary tracts later became loyalists. The result was that after 1774 the "Loyalists were simply crystallizing and expanding upon ideas which had been for some time an integral part of colonial political culture" (pp. 62–63).

In addition, like the popular party, the loyalists also derived ideas from the political rhetoric current in the parent state. The sinister tone of the Tory message was garnered from this source, she suggests, and so too was much of the conspiratorial logic and the hyperbole in the loyalist tracts.

The loyalists, according to Potter, were not reactionaries. They did find fault with the British empire, and they did recommend alterations in its structure. But what they generally called for was a tightening of imperial controls. Some hoped that colonials would be represented in imperial councils, but not even these Tories advocated any abridgment of imperial powers. By no means, she argues, were the loyalists anxious to secure dominion status for the colonies. "What they proposed," she adds, "was more like a federal union spanning the Atlantic" (p. 167).

There are one or two weaknesses in Potter's account. She has little interest in economic factors as a source for loyalist behavior or thought, though one does not have to read many Tory polemics before their class biases—and their fear of the "common sort"—become quite apparent. In a study of this sort, moreover, Potter should have devoted more attention to the question of the ineffectiveness of loyalist ideology. Having taken pains to show that it was derived from many of the same sources as the Whig philosophy, it is important to examine in detail why one ideology triumphed and another failed.

These shortcomings aside, Potter has written an important, at times provocative, book. Readers will find this to be a handsome, readable product, one that is replete with an exhaustive set of notes, albeit, lamentably, devoid of a bibliography. Readers addi-

tionally will find a fresh reminder of the fate that sometimes befalls a self-contained governing class that endeavors to understand sweeping changes by harkening to traditional beliefs and values, and, for its obdurate myopia, crumbles in the ensuing upheaval.

JOHN E. FERLING
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DAVID MCLEAN. *Timothy Pickering and the Age of the American Revolution*. (Dissertations in American Biography.) New York: Arno. 1982. Pp. viii, 511.

Timothy Pickering is best known as a partisan zealot who helped sabotage the Adams administration and later emerged as a leading New England reactionary. David McLean's new biography suggests that Pickering, in his earlier incarnation as American Revolutionary, was a much more admirable and sympathetic character. Driven at first by a "conscientious commitment" to town and colony (p. 43), Pickering grew with the Revolution to become a cosmopolitan nationalist. In his "selfless devotion" to the cause, Pickering epitomized republican virtue, sacrificing the comforts of private life and prospects for personal wealth. Pickering was "ascetic in his private life," while exemplifying "the ideal of public virtue in the impressive sacrifices he made to the common cause" (p. 350). Unfortunately, McLean does not make a strong case. While the details of Pickering's early public career are carefully recounted, his motivations remain obscure. Pickering is not the exemplary man McLean thinks he is, and this rendition of his life tells us little new about the age of the Revolution.

The most useful chapters concern Pickering's wartime service—as adjutant general, commissioner of the board of war, and quartermaster general—and his subsequent career as a Pennsylvania official in the troubled Wyoming Valley and as federal Indian negotiator. He was an honest and efficient, if unimaginative, administrator: unlike many of his colleagues, he did not enrich himself through office. Pickering was rewarded for his services with federal appointments, beginning with postmaster general in 1791. When he joined the cabinet in 1795, few of his contemporaries could have anticipated his metamorphosis into a bitter partisan and controversialist.

McLean's enthusiasm for Pickering stands in sharp contrast to Gerald Clarfield's unsympathetic portrait in his *Timothy Pickering and the American Republic* (1980). To some extent the differences can be attributed to the scope of the two books. Clarfield attempts to give a coherent account of Pickering's entire career and thus minimizes what McLean sees as a crucial transformation in the 1790s. Because McLean wants to distinguish the Revolutionary

from the High Federalist, he takes great pains to establish Pickering's Whig credentials in the imperial crisis. McLean claims that Pickering's conservatism—his belief in balance, order, and hierarchy—were broadly shared by most Revolutionaries; he rejects Clarfield's contention that Pickering's politics were symptomatic of narrow class interests and class consciousness. But while McLean discounts a materialist explanation for Pickering's behavior—and pays scant attention to his private life and personal development—he fails to offer a plausible alternative. The interpretative difficulties are most striking when he tries to connect the two phases of Pickering's career. According to McLean, Pickering was unable to sustain his characteristic balance between "intensity" and "moderation" in the 1790s: his "rationality gave way to hysteria" and "paranoia" (pp. 353, 364). Perhaps, McLean speculates, Pickering succumbed to "some psychiatric disorder" (p. 381). But if the later Pickering is disordered, we still have no explanation for his earlier behavior. McLean repeatedly insists that Pickering was not ambitious and that he did not share "the lust for fame" of so many others in the founders' generation (p. 131). Where then did his "strong sense of public duty" come from (p. 133)?

It would be easy to dismiss Pickering as an inveterate placeholder unable to make a living out of office. Yet such an explanation is no more satisfactory than McLean's depiction of Pickering as archetypal, virtuous republican. No man lives by bread—or platitudes—alone. To understand the Revolution we need to understand why men like Pickering embraced the cause: McLean's biography offers few clues.

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IMOGENE E. BROWN. *American Aristides: A Biography of George Wythe*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1981. Pp. 324. \$32.50.

Imogene E. Brown's central argument is an easy one to accept. Surely George Wythe (1726–1806) deserves more attention than he has yet received from historians and the general public. After all, he did stand with the giants of Virginia's great generation, and his signing of the Declaration of Independence was only one of the many highlights in a long and eventful career.

At different times Wythe held virtually every political office in colonial Virginia, and he participated in Revolutionary meetings, the Continental Congress, the Constitutional Convention of 1787, and the ratification assembly a year later in his native state. In a calm and reasoned manner he took an advanced position on some of the most contro-

versal questions of his day, notably in his opposition to a stamp tax, his advocacy of a break with England, and his criticism of slavery. Through it all he was widely admired for his erudition, wisdom, and personal rectitude.

In the legal field he towered above most of his contemporaries at the bar, and he became the nation's first professor of law, at the College of William and Mary, where he introduced the pedagogical devices of the moot court and mock legislature. He contributed to a root-and-branch revision of Virginia's code, and he served in judicial capacities from the county level to the chancellorship of the commonwealth. Among the many prominent Virginians instructed by him as youngsters were Thomas Jefferson (who regarded Wythe as a foster father), John Marshall, and Henry Clay.

Unfortunately, Brown's biography falls short of its distinguished subject, and has the earmarks of a labor of love more than of fresh or authoritative scholarship. The book provides a straightforward overview of Wythe's life and is judicious if admiring in tone. But the volume suffers, as have others, from a paucity of Wythe's personal papers, a problem compounded in this instance by other flaws. The author's research seems to have been done a decade or more ago, and thus she largely ignores the recent outburst of theses, articles, and monographs on late eighteenth-century Virginia. The text is further marred by chunks of undigested source material and by numerous factual and typographical errors. Except for an appendix on the Fawn Brodie allegations, there is little in content that cannot be found in William Clarkin's *Serene Patriot: A Life of George Wythe* (1970)—a title missing from Brown's slim bibliography—or in the scholarly writings of W. Edwin Hemphill.

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E. STANLY GODBOLD, JR. and ROBERT H. WOODY. *Christopher Gadsden and the American Revolution*. Knoxville: University of Tennessee Press. 1982. Pp. xi, 302. Cloth \$24.95, paper \$12.95.

Christopher Gadsden was "naturally stubborn" (p. 206). When the British governor of East Florida demanded his parole in 1780, Gadsden refused and spent the next forty-two weeks in solitary confinement at St. Augustine. This dramatic event was merely the climax of a whole series of stubborn actions that characterize this fascinating biography.

After an initial chapter, in which Gadsden's formative years are recounted in an engaging style, E. Stanly Godbold, Jr. and Robert H. Woody examine the Charlestonian's contributions to the American Revolution. As South Carolina's delegate to the

Stamp Act Congress in 1765, Gadsden voiced the radical opposition to imperial policy. Assuming leadership of the Sons of Liberty in Charleston, he spoke and wrote passionately (and interminably) about all the errors of the imperial government, including Townshend's and North's tea taxes. Elected twice to represent South Carolina at the Philadelphia congresses, he espoused the cause of independence while more moderate forces were still working for an accommodation. In fact, Gadsden only signed the "Olive Branch Petition" of 1775 on the assumption that rejection by the imperial government would strengthen the position of American radicalism (p. 135).

Gadsden's views appealed to the mechanics of the port city and to the unenfranchised of the back country, but at the same time he alienated moderate spokesmen such as Henry Laurens and John Rutledge. When the British eventually captured Charleston in 1780, Governor Rutledge was leading a government in exile, while Lieutenant Governor Gadsden reluctantly accepted the need to surrender. After he returned from imprisonment, he continued to speak and write on important issues. Even in failing health, he urged adoption of the federal constitution and greater participation in state government by back-country men.

Although Gadsden occasionally endorsed political positions that may have favored his business affairs, these authors do not suggest economic determinism as his motive. He genuinely believed, if somewhat intemperately, that imperial policies after the Stamp Act were unconstitutional. Unfortunately, irrefutable documents are not always available to substantiate their interpretations and they resort to "probably" many more times than students are usually permitted to do. (For an example, see the passage concerning Gadsden's putative design of the Rattlesnake Flag [p. 142].) The conjectures, however, are always based on extensive research and are quite unexceptionable.

The critical apparatus contained in the notes is so impeccably admirable that one must forgive the publisher for placing all the notes at the end of the narrative, instead of at the bottom of each page where they would have been even more valuable. Although reviewers should generally avoid the word "definitive," it is very difficult to believe that anyone will write another, much less better, biography of Christopher Gadsden.

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JOSEPH W. PHILLIPS. *Jedidiah Morse and New England Congregationalism*. New Brunswick, N.J.: Rutgers University Press. 1983. Pp. x, 290. \$27.50.

This is a solid account of Jedidiah Morse's career. Born in Connecticut in 1761, Morse became the epitome of New England orthodoxy and an arresting embodiment of Puritanism gone thin and sour. Morse the man hardly makes an appearance in the book that focuses instead on the major events in his career as a publicist and strategist for the New England ministry at a time when both New England and the orthodox Congregational clergy were losing ground to other regions and religions. We watch Morse as he graduates from Yale in 1783, studies theology with Jonathan Edwards, Jr., writes and promotes the *American Geography* (1789), plays a leading role in turning the New England clergy against the alleged democratic excesses of the French Revolution in 1796, founds and edits the *Panoplist* (1805) as the chief organ for moderate Calvinism, tries vainly to prevent Harvard from being captured by Unitarianism, supports the establishment of Andover (1808) as a bastion of orthodoxy, involves himself in missionary efforts aimed at sending blacks back to Africa and Indians onto reservations in the Northwest territories, and dies insolvent, unloved, but respected in 1826.

Joseph W. Phillips reviews this story ably. The book is strongest in explaining the religious context in which Morse worked, dissecting the differences among Morse's brand of Congregational orthodoxy, the evangelical movement that surfaced in the Second Great Awakening, and the liberal values of Unitarians in Boston. Three intriguing themes of scholarly significance emerge. First, Phillips demonstrates that Morse was a central figure in the creation of what became the antebellum benevolent empire, imitating the tactics of evangelical tract societies in England to spread the gospel according to New England throughout the new nation. Second, Morse emerges as one of the first ministers to base his authority on his skills as an organizer and publicist of Christian causes rather than a theologian or a pastor with deep roots in the local community. Third, Phillips is persuasive and original in his treatment of Morse's realistic brand of conservatism. Morse is usually regarded as a strident and uncompromising reactionary who saw democracy, atheism, and immorality as three loathsome elements in the same diabolical plot. But Phillips shows that he was also a cunning strategist who broke with the Federalists just before they became politically irrelevant, linked moderate New England ministers to the burgeoning evangelical movements in the South and West, and even accommodated himself to Jacksonian Democracy when it became clear that it was here to stay.

The endnotes reveal an exhaustive knowledge of the religious pamphlets, but the proportions of the book too accurately reflect the available evidence rather than the intellectual significance of the ideas

in them. And like many revised dissertations, the shuffling of notecards remains audible in the background. Morse's *American Geography*, which rivaled Noah Webster's speller in sales and cultural importance, is described but not analyzed. Nor is it any longer adequate to explain Morse's near-paranoid hostility to the Democratic Clubs and the "Bavarian Illuminati" as the result of "a deeply suspicious nature" (p. 226). For Morse's obsessive fears to become credible and comprehensible, they must be set against the background of profound social and attitudinal changes sweeping through America from 1790 to 1820. Otherwise, Morse becomes just another idiosyncratic, albeit shrewd, reactionary who manages to combine the most offensive qualities of Cotton Mather, John Adams, and Noah Webster. Although Phillips does a splendid job of explaining the religious context in which Morse worked, he ignores the social context altogether; nor does he ever get inside Morse's mind to show us what all the worry about imminent degeneration was about. Most biographers take their subject too seriously. Phillips, who acknowledges his distaste for Morse, seems to have erred on the other side. Nevertheless, this new biography immediately supercedes the two earlier, and now quite antiquated, accounts of Morse's life to become the standard one for this generation of scholars.

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LEONARD I. SWEET: *The Minister's Wife: Her Role in Nineteenth-Century American Evangelicalism*. Philadelphia: Temple University Press. 1983. Pp. viii, 327. \$29.95.

Career choices, for women as well as men, are a reflection of the values that counted at a particular historic moment, coupled with the possibilities available to the educational, economic, and aesthetic group making the choice. The role of minister's wife became "a coveted career" to which young American women aspired, albeit with a sense of their own unworthiness. Leonard I. Sweet has identified four thematic roles for this "elite" group: the companion, who was the ministering angel to her husband's pastoral ministry; the sacrificer, whose contribution was the negative one of not hindering the sacred calling of her husband, and dealing with the mundane details of parsonage life on her own; the assistant who helped, always in a rhetorically ancillary way, with his responsibilities and functions; and the partner, who not only had her own pastoral work but served as the pastor's own pastor. Sweet deals with these models chronologically, and, to some extent, denominationally.

The machinery of Evangelical conversion, its "ge-

stalt," enabled women to participate in the central and critical aspect of the movement, thus eroding any lingering sense of a male-dominated theology and clergy. A particularly interesting chapter discusses the way in which the ambition (itself always suspect in a female) to become a minister's wife both built on traditional female roles and expanded them. He finds that the careers of women preachers in the first half of the century took a terrible toll, physically and mentally. This, of course, might well be said of male ministers and missionaries, no fellows to take a back couch when hypochondria and psychosomatic illness were concerned. His conclusions stress the fear of the clergy that the energetic and successful minister's wife might embark on her own career. The two-person career, where both parties labored diligently in the field, although the title went to the male, was acceptable. The two-career marriage, with its tacit assumptions of equality in the eyes of God and man, was not.

The book is thoroughly researched, coherently presented, and original in interpretation. It is scholarly in its use both of original and secondary sources, and violates no canons—ideological or academic—in its conclusions. It is social history about women, in which the historical record is augmented and illuminated, if not radically altered.

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ROBERT ERWIN JOHNSON. *Far China Station: The U.S. Navy in Asian Waters, 1800–1898*. Annapolis: Naval Institute Press. 1979. Pp. xii, 307.

WILLIAM N. STILL, JR. *American Sea Power in the Old World: The United States Navy in European and Near Eastern Waters, 1865–1917*. (Contributions in Military History, number 24.) Westport, Conn.: Greenwood Press. 1980. Pp. xi, 291. \$29.95.

Throughout most of the nineteenth century the peacetime naval policy of the United States called for the deployment of warships on distant stations throughout the world. The purpose was to show the flag, to protect the lives and property of American citizens, and, above all, to further American commercial interests. Gideon Welles, Lincoln's secretary of the navy, best stated the rationale: "The commerce and navy of a people have a common identity, are inseparable companions. Wherever our merchant ships may be employed, there should be within convenient proximity a naval force to protect them." These two books—Robert Erwin Johnson's *Far China Station* and William N. Still, Jr.'s *American Seapower in the Old World*—chronicle the respective roles of the East India squadron (after 1866, the Asiatic squadron) from the early 1800s to 1898 and

the European squadron from its creation at the close of the Civil War to 1917.

Both are primarily operational histories; indeed, neither author intended to examine the larger issues of national or naval policies. Both illuminate many aspects of the social history of navies. Johnson touches on the way in which the navy dealt with disease, ranging from cholera to syphilis (on one voyage over half of the sick on the *Idaho* suffered from some venereal disease), on the problem of drunkenness (not always confined to the enlisted ranks), and the ever present difficulties of recruitment (post-Civil War Americans were so loath to enlist that the Asiatic squadron had to resort to recruiting foreigners of all nationalities). *American Seapower in the Old World* highlights the fascination of the Riviera and its social life for naval officers who, despite numerous protests from Washington, long preferred Villefranche, a few minutes from Monaco and Nice, for its winter base. And both books underscore the decline of the navy after 1865. The squadrons were generally miniscule, the ships outmoded and often out of commission—"they belong to a class of ships," as the *Nation* put it, "which other governments have sold or are selling for firewood"—and the navy department so penurious that in 1869, to save money, orders were given to use sail rather than steam except "under the most urgent circumstances."

Johnson's study of the Asiatic squadron, based primarily but not exclusively on its own records, is narrowly focused; it is a chronological narrative that concentrates on the experiences of individual ships and the officers who commanded them. Descriptive rather than analytical, *Far China Station* not infrequently lapses into naval antiquarianism. Although Johnson is occasionally critical of individual officers and believes that the often heralded achievements of Kearny in China and Perry in Japan have been exaggerated, he is, on balance, no critic of the navy or the policies that sent it to Asia.

Still's approach to the European squadron, on the other hand, is more detached, topically organized, and far more selective. Based on a richer selection of materials from both American and European archives, *American Seapower in the Old World* is a broadly conceived monograph firmly embedded in the context of American diplomatic, commercial, and naval development. (His summary, for example, of the evolution of the general board, is a model of concise writing.) Thus there is irony in comparing the two books. In the Pacific, where the interests of the United States were expanding, the navy was often directly involved in events of long-term significance—the opening of the treaty ports in China, the establishment of relations with Japan, and, eventually, the acquisition of territory. By contrast, the importance of the European squadron (at least in

European waters) declined almost from the moment of its creation. Yet the Johnson book contains little that is new on the principal diplomatic and naval events of the century, while the Still volume presents fresh information on such familiar episodes as the Perdicaris affair and opens a new chapter on American relations with the Ottoman empire.

What gives particular significance to the Still volume is the fact that the European squadron included the Mediterranean in its area of operations. And while American commercial interests in the crumbling Ottoman empire were virtually nonexistent, the missionary movement in the Near East, sponsored largely by Presbyterians and Congregationalists, was in full flower and possessed a political clout with which Washington—and the navy—had to reckon. Indeed, missionaries probably had more impact on Near Eastern policy than they did on American policy in China. Hence, in the fifty years covered by the Still volume, the European squadron spent more time in the eastern Mediterranean, essentially on missionary business, than in any other part of the European station. The sailors did not always approve. Their own investigations often led them to believe that the missionaries exaggerated threats to their security and that the American diplomats who called for a naval presence were alarmists responding to political pressures from Washington. "I believe that the wayward Turks are accused of having roasted either a stray missionary or possibly boiled a stray consul," a disgusted Lieutenant Charles Sperry complained, "and we are supposed to proceed to Constantinople and wring satisfaction from the Sultan." Admiral Kirkland, as acerbic as he was outspoken, once called the missionaries "a bad lot" and confided to Alfred Mahan that "I met several liars and sons of bitches in the east, not all of them outside the limits of the Church." Kirkland, who further outraged missionary opinion by his opposition to stationing naval units in Turkish waters, was soon recalled. More importantly, Still's analysis of the American response to the Armenian massacres of 1895–96 suggests that the episode should be ranked with Cuba and Venezuela in fanning the flames of jingoism.

But the United States still operated on the periphery of world events in the years covered by these two volumes. Johnson's volume clearly underscores the less than heroic role that America played in dealing with the opium traffic in China; the one exception to his general approval of the American stance in Asia is his description of the tactics of negligence and "deceit" on the matter of opium. His book also points up the extent to which American diplomats and naval officers in China simply capitalized on the advantages that superior British naval power had wrested from the hapless Chinese. The European

squadron had no major role except in the Mediterranean and, even there, its normal strength was such that at the most it could offer only "refuge and shelter." While the squadron practiced "gunboat diplomacy," the United States did not become involved in the political events that presaged the breakup of the Ottoman empire. It is even doubtful if the naval presence, except on rare occasions, had any influence on Turkish behavior. On the other hand, Still does demonstrate convincingly that, by the turn of the century and as the new navy began to emerge, European attitudes toward the American naval presence began to change as Europeans started to suspect that an emergent United States was about to begin interfering in the Old World. Moreover, although the number of naval episodes may, by European standards of imperialism, have been minimal, both books clearly suggest that the difference between Americans and Europeans in the Mediterranean and Far East was one of degree and not of kind. The Palmerstonian insistence on protecting the rights of American citizens in so-called "backward" countries, the attitude that Asians and Ottomans were heathens to be disciplined into obedience, and, above all, the latent desire to export American power and further commerce were—well before the rise of the new navy and the gospel of Mahan—considerably more than trace elements in American history.

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ALISON GOODYEAR FREEHLING. *Drift toward Dissolution: The Virginia Slavery Debate of 1831–1832*. Baton Rouge: Louisiana State University Press. 1982. Pp. xiv, 306.

DICKSON D. BRUCE, JR. *The Rhetoric of Conservatism: The Virginia Convention of 1829–30 and the Conservative Tradition in the South*. San Marino, Calif.: Huntington Library. 1982. Pp. xxi, 218. \$18.00.

Within a period of less than three years beginning in 1829, Virginia was the scene of a notable state constitutional convention, a slave revolt of unmatched ferocity, and a remarkable legislative debate on the future of slavery in the state. These events took place at a time when Virginia was well advanced in the process of becoming two different and discordant worlds: the old Tidewater-Piedmont dominion, where the streams flowed into Chesapeake Bay and black faces outnumbered white ones, and the Trans-Allegheny region, where the streams flowed toward the Ohio River and slaves were relatively few. In the middle was the Great Valley, pulled in both directions but more likely to associate itself with the western interest. Together, the valley

and Trans-Allegheny already held 46 percent of the state's white population in 1830, but the Tidewater and Piedmont remained dominant in a political system structured in their favor. The series of political struggles in Virginia from 1829 to 1832 were therefore a continuing contest between East and West, between old and new, between conservatism and reform, between slavery and antislavery, between a plantation establishment and outsiders demanding their fair share of political power.

The Rhetoric of Conservatism is a study of the convention of 1829–30, in which Virginians (including James Madison, James Monroe, and John Marshall) came to grips with the twin problems of representation and suffrage. More specifically, Dickson D. Bruce, Jr. concentrates on the rhetoric of convention conservatives, viewing it both as an instrument of persuasion and as an expression of a venerable political culture. In an analysis of roll-call votes, he finds that of the ninety-six delegates, forty-two (all from the Tidewater and Piedmont) voted conservatively at least 80 percent of the time, and thirty-nine (largely from the valley and Trans-Allegheny) supported reform with similar frequency. That left fifteen men who were somewhat less consistent—nine “moderate conservatives” and six “moderate reformers.” It was to this middle group that conservative speakers addressed themselves, and they did so primarily, says Bruce, by invoking certain general themes of the conservative tradition. That is, they stressed the weakness of human nature, the fragility of social order, the dangers of democracy (as exemplified in the French Revolution), and the riskiness of change. Their strategy proved successful enough to frustrate the principal desires of the reformers. The new constitution liberalized but did not abandon property qualification for voting; it conceded legislative apportionment according to the white population, but fixed the census of 1820 as the basis of calculation, with an inadequate provision for subsequent change. The result was a clear-cut victory for the slaveholding East and the status quo. Bruce's analysis of conservative rhetoric, although it sometimes seems to be an elaboration of the obvious, nevertheless contributes appreciably to one's understanding of the convention.

In *Drift Toward Dissolution*, the final version of a PhD dissertation completed in 1974, Alison Goodyear Freehling devotes one chapter to an excellent summary of the constitutional convention, but her main concern is with the legislative debate on slavery that followed in January 1832. That debate, inspired primarily by the Nat Turner uprising, was intensified by Trans-Allegheny dissatisfaction with the slaveholding elite's continuing domination of public affairs. The division in the House of Delegates was similar to that in the constitutional conven-

tion, and the outcome was also much the same. Antislavery forces, largely from the valley and farther west, showed surprising strength and won some vague rhetorical concessions but failed in their major purpose when the house resolved that it was “inexpedient for the present legislature to make any legislative enactment for the abolition of slavery.”

Freehling's thorough scholarship is somewhat compromised by her strenuous pursuit of a revisionist thesis. Historians, she believes, have habitually misinterpreted the slavery debate of 1832 as a major turning-point in Southern history when Virginia repudiated its Jeffersonian antislavery tradition and embraced the proslavery philosophy of the lower south. Freehling finds instead an antislavery continuity in Virginian history from the Revolution to the Civil War. Jefferson, she maintains, never deserted his youthful antislavery ideal and neither did Virginia. The act of 1806 restricting private manumission, for example, did not reflect a “new proslavery zeal” but rather brought state law into line with abolitionist-colonizationist principles. As for the great debate of 1832, its most important feature was an underlying consensus. No one defended slavery as a positive good, perpetually desirable. On all sides there was agreement that Virginia would some day be a free state and that removal of slavery must include removal of blacks. Abolitionists and “conservatives” clashed over means, not ends. The latter rejected abolitionist demands for a program of *post-nati* emancipation as unjust and impractical but at the same time anticipated the eventual disappearance of slavery as a “natural” consequence of the domestic slave trade. And, contrary to standard historical accounts, the antislavery forces emerged from the debate with a partial victory in the form of a preamble that looked forward to a time when abolition would begin. Thus, according to Freehling, Jefferson's vision of a Virginia gradually cleansing itself of blacks and slavery was reaffirmed. But realization of the vision depended on the progress of efforts to colonize Virginia's blacks abroad, and with the failure of colonization, largely because of free black resistance, the antislavery ideal lost much of its force in Virginia, though it never disappeared.

Although Freehling's revisionist argument lends zest to the book and will no doubt serve as a healthy corrective to certain stereotypes about antebellum Virginia, the persistent note of special pleading tends to impair the reader's confidence in the author's judgment. Her brief for Jefferson's lifelong consistency as an antislavery champion ignores or glosses over too many hard facts. In viewing the outcome of the great legislative debate as, on balance, an antislavery victory, she places far too much reliance on the vague rhetoric of what proved to be a meaningless preamble. And there is something

perverse about stressing the agreement rather than the disagreement between men who wanted to set Virginia immediately on the road to abolition and men who insisted that the problem must be left to beneficence of time, aided by the domestic slave trade. Still, one can read this monograph simultaneously quarreling with its thesis and learning a great deal from its scholarship.

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ALLEN KAUFMAN. *Capitalism, Slavery, and Republican Values: Antebellum Political Economists, 1819–1848*. Foreword by ELIZABETH FOX-GENOVESE and EUGENE D. GENOVESE. Austin: University of Texas Press. 1982. Pp. xxx, 189. \$25.00.

This is an explicitly Marxist critique of the work of three antebellum American political economists and, as background, of the ideas of Adam Smith, Thomas R. Malthus, and David Ricardo. In a twelve-page foreword Elizabeth Fox-Genovese and Eugene D. Genovese present Allen Kaufman as joining “a swelling group of younger Marxist historians who are illuminating Southern intellectual history and the ideological development of the slaveholding class” and conclude that “Kaufman makes his case for the ideological aspects of economic thought and, in so doing, makes his case for the advantages of Marxist method” (p. xix).

The author reports that he selected treatises that are considered or seemed to him “to be the most rigorous formulations of a particular position” for his discussion of “ambiguities or logical inconsistencies” (p. xxviii). His specific intent is “to present systematically the legitimating concepts within the discourse and, where possible, to show their connection to the structure of production on which the political economists reflected” (p. xxix). Kaufman expresses a hope that political economy, so retrieved, will be “an interpretive tool for reevaluating the political equilibrium and disequilibrium of the antebellum period” (p. xxix). Although he acknowledges that this will require a separate study of the political and constitutional debates preceding the Civil War, he returns to the theme in an afterword: “Once the competing definitions of concepts like nature, freedom, and the republic are given economic—which is to imply moral—meanings, it should be possible to understand how the economic debates helped structure the political and constitutional crisis of the 1850s” (p. 137). This book is a beginning at giving economic meanings to some of the concepts at issue.

The study is primarily a painstaking and rigorous exegesis of writings by the six subjects, informed by relevant narrative history of contemporary develop-

ments. Kaufman considers the work of the three Americans as attempts to “reconcile the early stage of economic growth to the long-term outcome that brings degradation to labor and the potential threat of a radicalized working class” (p. xxx). The single representative of the American North is Daniel Raymond, whose major effort appeared in the early 1820s. Virginian Thomas Roderick Dew published chiefly in the 1830s, and South Carolinian Jacob N. Cardozo from the 1820s into the 1840s. Kaufman’s extensive and illuminating exegesis and critique defies summarizing, though not surprisingly he judged all three to be hopelessly unsuccessful in reconciling capitalism’s prospective development with either Northern or Southern projections of a good society. Questions will persist about the representativeness, or even significance, of the three Americans despite Kaufman’s sustained defense of his selections. His intellectual mastery, however, makes each essay valuable on its own terms. How useful this foundation may become cannot be confidently predicted until further interpretive structure is erected.

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JEFFERY ROSSBACH. *Ambivalent Conspirators: John Brown, the Secret Six, and a Theory of Slave Violence*. Philadelphia: University of Pennsylvania Press. 1982. Pp. xii, 298. \$23.50.

Jeffery Rossbach’s study of the six men (two Unitarian ministers, a teacher, a physician, and two businessmen) who financed John Brown’s 1859 raid raises as many questions as it answers. *Ambivalent Conspirators* addresses not only why “six nominally pacifist abolitionists decided to support slave violence at Harpers Ferry” (p. 4) but also “why individuals seek violent means to affect [sic] political change” (p. ix). Approaching these quandries largely through psychobiographical observations, Rossbach, who is associate editor of the *Black Abolitionist Papers*, concludes that the common motivation of the “secret six” lay in their ideological transition from “romantic racial notions” that Africans were inherently “docile [and] pliant” (p. 8) to a new belief that slaves would, if given the chance, fight for freedom. Yet this explanation is undermined by the author’s own evidence that Gerrit Smith and Thomas Wentworth Higginson had both advocated physical resistance to slavery from their first espousal of abolition although, conversely, Theodore Parker lauded Afro-Americans’ good-natured, sambo personalities even after he had given Brown his support.

Although its central thesis is unconvincing, this

book is, nonetheless, an important one. In charting the courses by which five respectable Yankees and a wealthy upstate New Yorker conspired treason and insurrection, it contributes to the ongoing debate about how—or whether—psychobiography enriches historical understanding. One may challenge Rossbach's underplaying the force of public events, especially the frustrating interaction between the 1850 fugitive slave law and Bleeding Kansas on the one hand, and weak government and unresponsive politics on the other. One may also miss references to the increasingly violent rhetoric by which Northern politicians and abolitionists expressed that frustration. But Rossbach is, after all, interested in those who went beyond rhetoric, not so far as to engage in violence themselves but far enough to promote the vicarious acting out by others of fantasies in which they, as well-educated (five of the six were college graduates), mid-nineteenth-century, upper-middle class, success-oriented men could scarcely indulge.

Rossbach's narrative, quotations, and analysis converge most convincingly when they focus on the conspirators' need to demonstrate their own—not slaves'—manliness. The data suggest that the forceful presence of strong women in the lives of five of them contributed to that preoccupation. George Luther Stearns, who had been reared by a widowed mother intent on furthering her son's career, married a drivingly ambitious wife. Only when Higginson's mother had taken charge after the death of her ineffectual husband when Thomas was still a boy, did the family prosper. Parker and his close friend Samuel Gridley Howe both married proper-tied and assertive women: Lydia Cabot Parker, as dedicated an abolitionist as her husband, and Julia Ward Howe, an aspiring writer restless in a marriage rent by quarrels over money and her career. Rossbach, furthermore, attributes the whole reform enthusiasm of Franklin Benjamin Sanborn to his cousin-fiancee's influence, even though she died while Sanborn was a Harvard undergraduate. In addition, three of the six were weakened by poor health. Smith's mental and physical collapse after Harpers Ferry, which sent him to the Utica Asylum, was similar to, albeit more severe than, previous hypochondriacal attacks. Parker's tuberculosis had exiled him in Italy even before the raid. And Howe's persistent illnesses were exacerbated by marital tensions.

Whether driven by physical disability, emotional stress, or both, the conspirators all expressed a need to demonstrate their manliness. Yet only Higginson ever actively engaged in antislavery violence, having led the 1854 attempt to rescue Anthony Burns in which a deputy marshal was killed. Of the others, Stearns, who worried lest his sons grow up "soft and effeminate" (p. 85), panicked after Harpers Ferry and fled to Canada, as did Sanborn, who in 1856

had failed to complete an official mission to Kansas because he was terrified by the prevailing violence. Howe, like Stearns, lied to a congressional investigating committee about his role in the raid that each had previously asserted turned treason into patriotism.

Higginson might lambaste the others for their cowardice, but it was each man's fear that he lacked the *virtu* or *machismo* Brown embodied that linked him to a man whose notoriety came from a bloody frontier massacre and whose plans encompassed guerrilla warfare and slave insurrection. For each conspirator, Brown was a surrogate acting out dreams inappropriate to the lives of urban intellectuals, professionals, or successful men of business.

By exploring their ambivalence about violence, Rossbach may also have illuminated the radical chic of the late 1960s and early 1970s that inspired his study. More substantively, he has demonstrated psychobiography's utility in discovering individual motivation. But how much has he, in developing a common motivation for six men involved in a single conspiracy, explained the forces that ultimately involved not just tens but millions of men in the violence of civil war?

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RONALD L. F. DAVIS. *Good and Faithful Labor: From Slavery to Sharecropping in the Natchez District, 1860–1890*. (Contributions in American History, number 100.) Westport, Conn.: Greenwood. 1982. Pp. xv, 225.

CRANDALL A. SHIFLETT. *Patronage and Poverty in the Tobacco South: Louisa County, Virginia, 1860–1900*. Knoxville: University of Tennessee Press. 1982. Pp. xvii, 159.

MICHAEL WAYNE. *The Reshaping of Plantation Society: The Natchez District, 1860–1880*. Baton Rouge: Louisiana State University Press. 1983. Pp. xii, 226. \$22.50.

Economic and social historians have discovered the postbellum South. During the past decade, they have begun analyzing such important issues as the origin of sharecropping and the crop lien, the effects of emancipation, and the source of the South's persistent poverty. Their work has generated a bitter debate (see *American Historical Review*, 84 [October 1979]: 970–1006). Using neoclassical theory and quantitative data, economic historians argue that the sharecropping and lien systems were rational products of a free competitive market. In the half century after emancipation, freedmen made significant progress. Unfortunately, the war and its

aftermath so badly shattered the regional economy that the South remained desperately poor. According to Marxist historians, however, sharecropping was part of a repressive labor system imposed by planters to perpetuate their wealth and power. As a ruling class with prebourgeois origins, planters blocked agricultural mechanization and economic diversification in favor of a labor-intensive agriculture, dooming the South to a future of poverty.

The three books under review shed light on various aspects of this debate, and they find weaknesses in both schools of thought. They all demonstrate the value of the local study as a historical tool. Ronald L. F. Davis and Michael Wayne both deal with the Natchez district, one of the wealthiest and oldest plantation regions of the cotton South. Crandall A. Shiflett examines postwar changes in Louisa County, Virginia, a tobacco county in the piedmont. Neither the Natchez district nor the Virginia piedmont can claim to be typical of the South, but they did reflect the social changes that affected the postwar period. The authors blend traditional sources with quantitative data drawn from federal population and agricultural censuses and from courthouse records. Lien, mortgage, and tax records prove vital for all three studies.

In *Good and Faithful Labor*, Davis deals with a narrow but important problem—the origin of sharecropping. He rejects the legend that it originated when planters and freedmen brought together land and labor in a mutual bargain necessitated by lack of cash or credit. Nor did planters introduce shares in order to tie the freedmen's earnings to the harvest and thus keep them at work. In a criticism of recent work by Robert Higgs and Joseph D. Reid, he also denies that sharecropping was a compromise between landlords and tenants, enabling both to share the costs and risks of production. Instead, Davis argues, it was the former slaves who wanted it, and they refused to work under any other arrangement. The freedmen insisted on sharecropping as a means of escaping the gang labor system that was associated with slavery and still practiced after the war. If they could not secure land of their own, they were determined to rent land and work in family units under their own supervision. "Planters," Davis adds, "literally were dragged kicking and screaming into the system" (p. 190). Contrary to other recent studies, Davis's research produces evidence that the planter class suffered major losses of land and status after the war. They were, in his opinion, no longer planters in the antebellum sense.

As Davis recognizes, other parties played a role in the eventual outcome of labor arrangements in the Natchez district. Although the U.S. Army and the Freedmen's Bureau did not promote sharecropping, they did enable freedmen to resist the move back to gang labor. As the freedmen's chief source

of supplies, the local merchants played a key role in the institutionalization of sharecropping. Since a first lien protected their investment, merchants were quite willing for the freedmen to work in family units without close supervision. In any event, the efficiency of the district's croppers compared favorably with that of slaves, so there was no "managerial reason" (p. 194) for a return to gang labor. Despite its later association with endemic poverty, Davis views the emergence of sharecropping as a victory for former slaves.

Davis, then, insists that a labor market functioned in the early postwar years and that it was a seller's market. Other studies have also found that planters had to respond to the freedmen's desires in the evolving labor arrangement. Davis is thorough and thought-provoking. The key weakness in his argument is that he can not explain why freedmen preferred shares over other forms of tenancy such as fixed renting. What freedmen apparently wanted was to avoid gang labor and low wages, and sharecropping was the best deal they could get. Perhaps they made a choice, but it was a negative not a positive one.

After Davis, Wayne's *The Reshaping of Plantation Society* appears to describe some other Natchez district. His purpose is to show how the antebellum planter class did indeed survive the upheaval of war and emancipation and maintain its interests. In the process, planters, freedmen, and merchants evolved a new set of relationships, creating a new plantation society. After emancipation, former slaves capitalized on the existing labor shortage by forcing planters to compete for their services. This exercise of economic power inaugurated a sweeping change in labor relationships. The paternalism of the old order collapsed, and the new order centered around the marketplace. In response, the district's elite supported the Black Codes as an effort to legislate the marketplace out of existence, some imported European and Chinese laborers, and others leased lands to Northerners. Most of all, they resisted renting lands to freedmen, but eventually they had to accommodate free black labor. Wayne and Davis agree that a seller's market operated at least initially after emancipation, but Wayne does not see sharecropping as a unilateral development. He seems to indicate that it was the result of a trial and error process that can be easily oversimplified and misunderstood.

In the transition to free labor, planters also faced a threat from local merchants, whose influence increased in tandem with the rise of tenancy. Since merchants possessed the supplies and credit freedmen needed, they challenged landlords for control over the labor force. According to Wayne, the economic depression of the 1870s forced merchants and planters into a partnership, drawing planters

back into the supply process. In the end, former slaveowners re-established their class supremacy, and after a period of relative economic power, former slaves found themselves again trapped in a state of subservience.

While Wayne emphasizes class conflict, he turns the Marxist interpretation inside out. He confirms the recent work of Jonathan M. Wiener and others on the "persistence" of the planter elite, but Wiener insists that planters retained their prebourgeois ideology, which explains their opposition to modernization. In Wayne's view, however, the introduction of the marketplace transformed the planters into a bourgeois ruling class. This interpretation indicates a sharp break between the Old South and the New. Wayne's major contribution is to show that planters and freedmen had to define new economic and social relationships. Unfortunately, he fails to put his conclusion in a larger context. We are not told how a bourgeois planter class affected the economic development of the Natchez district.

Shifflett's *Patronage and Poverty in the Tobacco South* is a social history of poverty in Louisa County, Virginia. The Civil War jolted the local economy, he finds, but it scarcely affected the social structure. Despite the destruction of slavery, the old aristocracy gained even more leverage over labor than before. In Louisa, slave paternalism gave way to a system of capitalism in which no free labor market actually functioned. Shifflett calls this system "patronage capitalism," a powerful form of class domination whose legacy was poverty and racism. It originated in both federal and state restrictions on the freedmen's mobility and bargaining power. But the enduring basis of the patronage system was the lien agreements that tied laborers into a complex hierarchy of work relationships. For most, the only effective way to challenge the system was migration. Although former slaves made remarkable progress in acquiring small tracts of land, Shifflett argues that land has been overrated as a measurement of wealth. The patrons still held a monopoly of the income-generating factors of agriculture—livestock, tools, and credit.

Unlike Wayne, Shifflett stresses the continuity between the slave South and the free South. Class rule characterized both periods. What the war changed was labor, not capital. Shifflett finds no intraclass conflict (such as planter versus merchant), nor does he see clients wringing any major concessions from patrons. Louisa was totally a buyer's market, probably the kind of society that Natchez planters hoped to create after the war. Shifflett also criticizes the claim that the planter class blocked agricultural mechanization. In fact, they gradually abandoned tobacco for less labor-intensive crops and increased their profits. As social history, Shifflett's work is impressive. Using studies of peasant

societies and New England towns as models, he ably reconstructs the social structure of Louisa and describes its effects on social conditions and social change.

Taken together, these books do not move the debate on the postbellum South any closer to a consensus. Most importantly they are divided on the question that goes to the heart of the debate—whether a free-labor market functioned after emancipation. Each writer examines only part of a topic or sees it from only one angle. They argue issues that are often more specious than real. For example, the question of continuity or discontinuity in Southern history has become largely a matter of emphasis. On many subjects, the large variability among people, places, and circumstances makes generalizations difficult. We need additional regional and local studies, but we need a new approach even more. Future studies should emphasize the behavioral approach of the new social history, focusing attention on people themselves and how they lived everyday life.

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DENNIS P. RYAN. *Beyond the Ballot Box: A Social History of the Boston Irish, 1845–1917*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1983. Pp. 173. \$24.50.

This book is the recipient of the first Fairleigh Dickinson University Press award for a manuscript in ethnic studies. It originated as a doctoral dissertation at the University of Massachusetts, Amherst. Its content is the nonpolitical history of the Boston Irish from the onset of the famine in Ireland to American involvement in World War I. The latter date is significant since Dennis P. Ryan considers the Irish to have adjusted successfully to Boston by 1917.

The heart of the text deals with a number of topics including poverty, schools (Catholic and public), small business and professions (law, medicine, journalism), interethnic relations (blacks, Jews, and Italians), Irish female domestics ("Bridget"), and leisure activities.

Ryan rejects the application of Andrew Greeley's sociological flashbacks to the Irish; the Boston Irish were not, in Ryan's view, "fatalistic and pessimistic." His conclusion rests on a convincing assessment of the Irish network of orphanages, asylums, benevolent societies, schools, and a college. The Irish constructed and staffed all of these during the period under review.

Though his chronological period is extensive, Ryan's content and conclusions remain largely with-

in the boundaries set by the works of Oscar Handlin and Stephan Thernstrom. Yes, it took the Irish three generations of slum living to find themselves and move ahead. And when they did, their control of jobs within local government and organized labor took them from poverty, but hardly by much. By settling for security most Irish never approached the higher world of Boston's economic, social, cultural, and intellectual life. Anti-Irish discrimination is examined, but Ryan does not consider it to be a critical factor governing the group's adjustment to Boston. Boston's adjustment to the Irish is not considered.

This is a well-executed book with abundant documentation, a comprehensive index, and well-chosen photographs. Its limitations spring from a paucity of text (101 pages). If, for example, the treatment of Bridget were half as ample as she herself was (see page 45), this chapter would constitute a significant contribution to the growing scholarly literature on the American Irish.

Interethnic relations represent a different problem. The reader is told that, in essence, Irish relations with blacks and Jews were harmonious; documented illustrations are given. Relations with Italians were less satisfactory. From this picture one has little historical background from which to assess subsequent racial problems as they relate to the Irish community of Boston.

To maintain that the Boston Irish were the brokers among immigrant-ethnic interests is correct. It is an appropriation of a conclusion, however, that applies generally to the Irish in American political history and is not the subject matter of this book. To maintain, further, that Irish confidence in the rightness and workability of a pluralistic society was a role consciously played by the Boston Irish is, I think, an overstatement. Like most groups, the Irish played a role they understood but dimly.

This is a fine product of scholarly research, one worthy of its prize. Yet, its conclusions should not extend beyond its content.

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MARILYN MCADAMS SIBLEY. *Lone Stars and State Gazettes: Texas Newspapers before the Civil War*. College Station: Texas A&M University Press. 1983. Pp. xiv, 405. \$21.50.

Between May 25, 1813, when *Garceta de Texas* first appeared in conjunction with the Bernardo Gutiérrez and Augustus W. Magee filibustering expedition, and the outbreak of the Civil War more than four hundred newspapers were published in Texas. In style and format they represented an extension of the Anglo-American press. Only a small number

would qualify as newspapers by modern standards. Most were established to support or to oppose a political candidate or an officer, particularly Sam Houston, some major issue, or a religious denomination. When the cause for its founding no longer existed or was superseded by another, the financial support proved insufficient, or the owners tired of the business, the paper was discontinued, given a new name, or moved to another town. Although those papers and their personnel have been the subject of many books and articles, this book is the first comprehensive, scholarly study to chronicle their history and to provide biographical and analytical information on all the well-known, as well as many lesser-known, editors, publishers, and owners.

Newspapers in Texas numbered ten in 1838, twenty-one in 1845, and eighty-nine in 1860. The *Morning Star* (Houston) was the first daily (1839). Only four established during the period of the republic survived the Civil War. Of those, the *Galveston News* was the most successful and enduring. The *Standard* (Clarksville) holds the record for the longest duration (1842–87) under a single editor-publisher.

Editors who achieved enduring recognition as journalists included Gail Borden, Jr., a strong supporter of Sam Houston; Francis Moore, Borden's successor at the *Texas Telegraph* (Houston) and the *Morning Star*; Hamilton Stuart, whose *Civilian* (Galveston) strongly supported Houston; Willard Richardson, who by the 1850s made the *Galveston News* the most widely circulated paper in Texas; Charles DeMorse, a moderate secessionist; Ferdinand Flake, a slaveholder but a strong Unionist, whose *Die Union* (Galveston) had the largest circulation in Texas in 1861; and John Marshall, who used the *Texas State Gazette* (Austin) to urge the organization of a strong Democratic party in Texas, the renewal of the African slave trade, and the withdrawal of Texas from the Union. Some preeminent publishers and owners were Gail and Thomas Borden, who started the *Telegraph*; Francis Moore; Michael Cronican, founder of the *Galveston News*; Jacob Cruger, who established the *Morning Star*; Charles DeMorse; and John J. Niles, who established the first newspaper in Galveston, Matagorda, Washington-on-the-Brazos, Austin, and San Antonio.

Marilyn McAdams Sibley has skillfully organized a voluminous amount of material and chronicled a complex story, filled with drama, with clarity and excellent balance. The writing (simple prose without wit, humor, or journalistic flair) and editing are both superb. An annotated checklist of Texas newspapers from annexation to the Civil War provides an additional contribution of inestimable value.

ERNEST WALLACE
Texas Tech University

TERRY L. SEIP. *The South Returns to Congress: Men, Economic Measures, and Intersectional Relationships, 1868–1879*. Baton Rouge: Louisiana State University Press. 1983. Pp. xii, 322. \$25.00.

This book is the most comprehensive analysis of the Southern delegation in the United States Congress during Reconstruction that has thus far appeared. Terry L. Seip carries his work beyond the election of Hayes to study the Southerners during the Forty-fifth Congress (1877–79) thus allowing a consideration of Hayes's Southern policy and the presentation of economic issues in broader perspective. In all, he analyzes the careers and legislative behavior of 251 men, excluding the Tennessee delegation because that state did not experience Congressional Reconstruction. Seip identified 122 Democrats and 129 Republicans, further subdividing the latter into 53 scalawags, 60 carpetbaggers, and 16 blacks. The author devotes two chapters to the social, economic, and political background and ideology of his subjects and one each to "the trials of election," and to "sectionalism and the politics of Reconstruction." Arguing that the Southern role in shaping national economic policy has been less well examined than issues relating to redemption, he devotes three chapters to such matters. In a final chapter Seip pulls the various threads of the study together.

Seip concludes that the Southerners "served their section well under extremely trying circumstances" (p. 293). Although their records of military service were in contrast, both the Southern Democrats and the Republicans had considerable prior experience in political office; more than 80 percent had previously served in state or federal political office. Seip stresses the chaotic nature of the campaigns, the physical dangers of the canvasses, and the high proportion of contested elections. Southern Republicans had, he writes, "no real political security and little outside support," and they "represented an intimidated, impecunious, faction-ridden party, saddled with a negative image which Northerners accepted with distressing quickness" (p. 111). In Washington they were denied their fair share of committee assignments and federal patronage. Although the Southern Republicans followed the party line on the issues of political Reconstruction, they joined their Southern Democratic colleagues very early in voting on economic issues. Such behavior first occurred in the late 1860s, rather than the mid- and late 1870s. Despite the keen interest of the Southerners in increasing the flow of federal currency and subsidies to the South, Seip believes that C. Vann Woodward's account of the Compromise of 1877 overemphasizes the importance of the Texas and Pacific railroad project. Southern Republicanism failed, argues Seip, because the Northern Republicans would not accept Southerners as equals as

well as because of disagreements over campaign aid, committee seats and Reconstruction policy, plus the waning of Northern radicalism. But also important were "differing sectional economic interests and the simple failures of Northerners to understand the political and economic needs of their southern colleagues" (p. 8).

Seip has used a broad selection of sources, both conventional and quantitative. In his analysis of biographical data and roll calls he has used the computer intelligently. This reviewer would have welcomed a more exhaustive electoral analysis, but only roll call specialists will fully comprehend the labor that went into this book. It represents a fine project, skillfully executed, and is presented clearly and forthrightly. It is a major contribution to the history of the Reconstruction era and the United States Congress.

ALLAN G. BOGUE
*University of Wisconsin,
Madison*

PAUL H. BERGERON. *Antebellum Politics in Tennessee*. Lexington: University Press of Kentucky. 1982. Pp. xii, 208. \$18.50.

In the case of Tennessee, Paul H. Bergeron disagrees with Charles S. Sydnor's contention that pre-Civil War Southern politics "had the hollow sound of a stage duel with tin swords" (p. 6). Whatever prompted a Tennessee voter to join the ranks of the Jacksonian Democrats or the Whig party (or its successors, the Know-Nothings and the Opposition party of the 1850s), he tended to maintain political constancy. Although the home state of Democratic Presidents Andrew Jackson and James K. Polk, Tennessee voted Whig in every presidential election from 1836 to 1852 and the Whigs won most gubernatorial contests during that period, but the statewide margin of victory was usually slight. After 1852 there was a slow trend toward Democratic hegemony; still the Opposition carried seven out of ten congressional districts in 1859, and the Constitutional Union party, with veteran Tennessee Whig John Bell as its standard-bearer, took advantage of the split in Democratic ranks to carry the Volunteer State in the presidential election of 1860.

Bergeron does not view the rival parties in Tennessee as representing distinctive socioeconomic or ethnocultural constituencies, although he maintains that "during at least part of the antebellum period town-based occupations inclined a voter to anti-Democratic ranks, while farming exerted an opposite pull" (p. 152). Based on his own quantitative analysis of state voting data as well as his use of recent statistical studies by other scholars, he concludes that "Certainly the notion that the Democrats

were the party of the 'plain folk' while the opposition harbored a disproportionate share of wealthy planters and merchants must be surrendered without hesitation" (p. 156).

Insofar as Tennessee is concerned, Bergeron regards William J. Cooper's thesis expounding the primacy of slavery in antebellum Southern politics as debatable. An admirer of Richard P. McCormick, he sees the state Democratic and Whig parties of the period as "essentially electoral machines, not given to notable ideological differences" (p. ix). Thus *Antebellum Politics in Tennessee* runs counter to the recent trend of many American political historians to give importance to the role of ideology. Unlike J. Mills Thornton III, who took ideology seriously in his study of neighboring Alabama, Bergeron makes no effort to relate pre-Civil War Tennessee politics to that State's secession in 1861. Noting that Tennessee voters responded in the presidential contest of 1860 pretty much as they had in recent statewide elections, he is content to conclude his study with the simple statement that "in 1861 things would be different" (p. 160).

EDWIN A. MILES
University of Houston

PAUL S. HOLBO. *Tarnished Expansion: The Alaska Scandal, the Press, and Congress, 1867-1871*. Knoxville: University of Tennessee Press. 1983. Pp. xix, 145. \$12.95.

In this short monograph Paul S. Holbo fails to persuade the reader of his major thesis but makes a significant contribution to the art of historical detection. It is Holbo's contention that the scandal and rumor of scandal surrounding the passage by the House of Representatives of the appropriation bill for the purchase of Alaska irreparably tainted the movement for territorial expansion in Gilded Age America, providing a major explanation for the frustration of later expansionist ventures in the Caribbean and the Pacific. The thesis is more provocative than persuasive.

Tarnished Expansion does provide, however, the most detailed examination to date of the suspicion of corruption that accompanied the payment of \$7,200,000 for Russian America, the campaign of rumor waged by various antiadministration newspapers, and the bumbling investigation of the House Committee on Public Expenditures. One can doubt that "the revelations in the press and the congressional investigation had an enduring effect on the course of American foreign policy" (p. xvii) and still admire the author's meticulous research and his narrative skill in describing a tale of suspected skullduggery that lacks a dramatic climax and forbids final judgment respecting the individual guilt

of its more prominent political figures. Holbo's hero is the investigative reporter Uriah Painter, and his villains are Robert J. Walker, Frederick P. Stanton, and Robert W. Latham; but he is determinedly fair and makes no exaggerated claims for his discovery of new material respecting the evolution of the Alaska scandal. His painstaking analysis adds significantly to the earlier accounts of William A. Dunning, Reinhard Luthin, and Richard Jensen.

Holbo is currently engaged in a major study of expansion in the post-Civil War generation with particular emphasis on "the relation between expansionism and scandal" (p. xvi). He believes that the charges of corrupt lobbying and secret payoffs to reporters and congressmen that erupted in 1868-69 during the battle over the Alaskan appropriation are important primarily because of their long-term influence on public attitudes. It was then that expansion and corruption became associated and that association was a crucial factor in frustrating the subsequent efforts of American expansionists to acquire Santo Domingo, Samoa, Hawaii, and Cuba. In the concluding chapter of *Tarnished Expansion*, Holbo seeks to persuade the reader of the validity of his general thesis by offering evidence of the influence of the Alaska scandal on the failing efforts of Seward and Grant to acquire the Dominican Republic. The connection demonstrated, however, is more chronological than causative. Surely there were charges of "job" and "fraud" in both instances, but Edwin Godkin and *The Nation* would not have supported Dominican annexation had every penny of the Alaskan purchase money reached the coffers of Czar Alexander.

One hopes that Holbo in his larger study will not emphasize the influence of the public association of expansion and corruption to the exclusion of other factors that help explain the failure of expansionist aspirations in the Gilded Age. Traditionalism, apathy, racism, and the nullifying effect of competing economic interests were perhaps more important than the fading public memory of the mysterious bagmen of Edouard de Stoeckl.

RICHARD E. WELCH, JR.
Lafayette College

ALICE E. REAGAN. *H. I. Kimball, Entrepreneur*. Atlanta: Cherokee. 1983. Pp. xiv, 166. \$9.95.

In Reconstruction historiography the carpetbagger has long borne a particularly odious reputation. That historians continue to employ the Southern conservatives' term of opprobrium to designate Northern Republicans who went South after the Civil War illustrates the persistent influence of the view that such men were political adventurers whose primary aim was plunder and exploitation. A few

decades ago Richard Current and others began to correct that biased view, and *H. I. Kimball, Entrepreneur* is a modest entry in the revisionist literature. Alice E. Reagan seeks further to correct the imbalance by giving due emphasis to the carpetbagger's legitimate economic enterprises as well as his political activity. According to Reagan, Kimball was not, as Claude Bowers suggested, the ringleader of a band of buccaneers, but instead a benevolent, albeit self-interested, businessman who had a sincere concern for his adopted city, Atlanta.

A native of Maine, Kimball early showed business energy, becoming a partner in a New Haven carriage-making firm in 1853, when he was twenty-one years old. During the Civil War he engaged in mining in Colorado but afterward returned east and became the Southern representative of the Pullman Palace Car Company. He selected Atlanta as his headquarters and soon turned to a host of other business ventures.

Kimball affiliated with the Republican party and became a close associate of scalawag governor Rufus Bullock, but he generally was on good terms with conservatives and rarely let politics intrude on his business affairs. His interests included railroad promotion, real estate, construction of the huge Kimball House hotel, and creation of the Atlanta Cotton Factory, the first such mill in the city. A subscriber to Henry W. Grady's New South philosophy, Kimball took the lead in organizing Atlanta's 1881 International Cotton Exposition and the International Commercial Convention four years later. Although his achievements were substantial, he had a penchant for jumping into grandiose schemes with insufficient funds or financial backing; with as many business failures as successes, he never accumulated a large personal fortune. Kimball won high praise for his accomplishments in Atlanta, but in the political arena they could not dilute the lingering sectional animosity that defeated his bid to become mayor of the city in 1880.

This book began as a master's thesis. Insufficiently revised, it still exhibits a certain lack of sophistication and infelicity of style that a good editor could have helped correct. Its primary significance is for the history of Atlanta.

CHARLES W. CALHOUN
Austin Peay State University

ROBERT S. MAXWELL and ROBERT D. BAKER. *Sawdust Empire: The Texas Lumber Industry, 1830-1940*. College Station: Texas A&M University Press. 1983. Pp. xv, 228. \$24.95.

It is conventional to associate economic life in Texas with cattle and oil wells and, in the years since 1960, with space age technology. Yet during a major

period of its growth (between 1880 and 1900) the lumber industry was the state's leading manufacturing enterprise and biggest employer. *Sawdust Empire* is the story of the bonanza years (from the 1880s through the Great Depression) of logging and lumbering activity in the East Texas Piney Woods. This well-researched study by Robert S. Maxwell and Robert D. Baker is a valuable contribution to the growing literature on forest history.

The growth of the East Texas lumber industry was directly associated with the post-Civil War construction of rail networks in the South that provided the means to transport logs from forest to mill and the finished product to major lumber markets. The book is arranged into chapters that describe the forest environment in eastern Texas, the earliest lumber mills in the region, the increasing efficiency and productivity of logging and milling technology, marketing strategies, the lives of workers in the mills and logging camps, the company towns, and finally an account of conservation practices and the declining importance of the industry as lumbermen harvested the old growth stands.

Sawdust Empire describes the flow of capital to the region as lumbermen in the Great Lakes states and the Northeast moved southward in the late nineteenth century to tap the virgin pine forests of the Gulf coast. New towns and camps appeared overnight as lumber entrepreneurs joined with railroad magnates to tap the great profits to be made in the Piney Woods. True believers in the industry's migratory habit—"cut-and-get-out"—lumbermen harvested the last commercially valuable stands of timber and then shifted their investments to other fields of activity or moved on to the last great old growth forest along the North Pacific coast.

Maxwell and Baker accurately describe the heavy handed paternalism of lumbermen like John Henry Kirby who ruthlessly worked to undermine their competitors and with equal force moved to destroy efforts to unionize their mills. Kirby's barony in the East Texas woods awarded mill managers a sliding salary scale based on productivity, insisted on the ten-hour day long after other operators had reduced their hours of work, and continued to use the notorious system of issuing merchandise checks rather than cash on paydays. In a final twist of irony, Kirby's far flung financial empire collapsed in the midst of the ruins of the Great Depression.

Despite the pernicious nature of this system, the authors find much to praise in these men who "created an industry, provided jobs, and organized a stable society where none had existed" (p. 153). Surely, the workers in the woods and in the mills would be hard pressed to find stability in a system that offered seasonal unemployment and repeated shutdowns when the market was slow. And, when the whistle in the big mill sounded for the last time,

the workers faced a devastated environment and an uncertain future.

Maxwell and Baker are at their best when telling the story of the bonanza years of Texas lumbering, especially the activities and personalities of the major lumbermen. The study is weakest in its assessment of the lives of working people and in its discussion of forest and conservation history beyond Texas. The American Forestry Association is mistakenly identified as a trade association (p. 160); the authors wrongly state that the federal government was unwilling to help boost the lumber export trade (p. 165); Charles Sprague Sargent (not "R. L.") wrote the major study of North American forests in 1884 (p. 168); and the authors are inconsistent in properly identifying the bureaucratic predecessors to the U. S. Forest Service (pp. 168–175).

WILLIAM G. ROBBINS
Oregon State University

JAMES H. DUCKER. *Men of the Steel Rails: Workers on the Atchison, Topeka, and Santa Fe Railroad, 1869–1900*. Lincoln: University of Nebraska Press. 1983. Pp. xxi, 220. \$17.95.

Although railroad workers were the largest single component of America's industrial labor force in the late nineteenth century, they have received relatively little attention from recent labor historians. Although there are numerous older monographs on railroad unions and strikes, James H. Ducker's study of Santa Fe workers is the first book to approach railroaders from the perspective of the new labor history that is interested in working-class culture as well as class conflict and workers' relations with their community as well as their employer. Organized topically, *Men of the Steel Rails* moves smoothly from opening chapters on job conditions, management policies, and "the romance of the rail," through discussions of community relations in a railroad town, the role of railroaders in politics, the protective and fraternal functions of the railroad brotherhoods, and community reaction to railroad strikes.

Although Ducker's description of the work environment is somewhat sketchy, coming largely from newspapers and union periodicals, he does a superb job of drawing on payroll, tax, and census records to demonstrate the geographical mobility of many railroaders and to show how that mobility affected their lives. We learn, for example, that because they moved about so much, the conductors and engineers of Emporia, Kansas, were no more likely to own homes than were common laborers, even though they earned two to four times as much money. Ducker's case study of community relations in Emporia bases rather sweeping conclusions on the scant evidence afforded by a few lists of lodge

members and officers. His treatment of railroader politics, on the other hand, is both thorough and enlightening. Ducker ably supports his conclusion that in Kansas the leadership of both railroaders and farmers was politically inept, but he also shows that there were conflicts of interest that precluded an effective political alliance between the two groups, as railroaders sought Populist backing for labor legislation but angered farmers by opposing governmental regulation of railroad rates. An analysis of twenty-six strikes along the Santa Fe leads Ducker to propose a model that deserves to be tested elsewhere: he finds that in small towns and in those larger communities where railroaders constituted a third or more of the total working population, they could generally count on sympathy and support from the local elite.

It is unfortunate that Ducker largely limits his focus to the Santa Fe mainline through Kansas, Colorado, and New Mexico, with only spotty comments on the rest of the Santa Fe system. The lack of access to internal management correspondence, although not the fault of the author, also detracts from his ability to tell the whole story of labor on the Santa Fe. Within these limits, however, *Men of the Steel Rails* succeeds admirably in recreating the world of the Santa Fe worker. Skillful in its blending of anecdotal and quantitative evidence, and judicious in its interpretations, this book offers worthwhile reading for historians of labor, the West, community development, and Populist politics.

DAVID L. LIGHTNER
University of Alberta

JERRY M. COOPER. *The Army and Civil Disorder: Federal Military Intervention in Labor Disputes, 1877–1900*. (Contributions in Military History, number 19.) Westport, Conn.: Greenwood. 1980. Pp. xv, 284. \$29.95.

Books on American labor history generally include some attention to the intervention of the military in strikes. Jerry M. Cooper's work, however, focuses on this issue, and primarily from the vantage point of the military. He is a keen enough observer to see the differences among the strikes, but nonetheless his stress is on a set of basic conclusions that are explored and expanded on as Cooper discusses the railroad strikes of 1877, the industrial armies and the railroad strikes of 1894, and the strikes by miners of the Coeur d'Alene region of Idaho in 1892, 1894, and particularly 1899.

Cooper argues that Federal troops were effective in every case in restoring order with only the most minor use of force. This intervention was also a major reason for the ultimate defeat of the strikers. Thus it was all the more remarkable that Federal

troops were not met with violence, as was the case many times when the National Guard confronted strikers. Cooper ascribes the lack of violence in part to the restraint of the Federal officers—men with experience in combat, or in civil disorders during Reconstruction; in part to the nonviolent intentions of union leaders; but ultimately to the fact that “Soldiers under the national colors, symbolic representatives of the nation as much as flesh-and-blood agents of law, simply were not to be fought” (p. 196). Was this nationalism a vital force for the new immigrants among the strikers? How does it explain the tremendous increase in verbal abuse and provocation of troops in the 1890s, but the continued refusal of strikers to turn to force? Cooper’s argument here is not convincing.

The author also stresses that the president and his cabinet officers offered no real leadership once troops were ordered into a strike. Combined with the poor command structure of the army, this left many decisions to field officers. Cooper believes their predilection for order, stability, and the rights of property, plus their hostility to unionism, were carbon copies of the views of the employers. It is thus not surprising that Federal troops became improperly involved in breaking strikes. Here Cooper draws too fine a distinction since the strike itself often was labeled as the cause of the disorder, and thus military officers had to use very little initiative to support management’s efforts against it. Moreover, as Cooper correctly points out, presidents from Hayes to McKinley fully shared these same views.

Some military officers tried to use public support for the army’s actions in these strikes to gain support for a larger army. This campaign failed as it ran against the traditional Anglo-American fear of a large standing army. Cooper ascribes the labor movement’s growing hostility in the 1890s to an expanded army as much to this shared value with other Americans as to working-class consciousness.

In sum, this book provides valuable descriptive material, and a generally sound and broadly conceived set of insights. Although marketed as part of a series in military history, it has much of value for labor historians, as well as for those generally concerned about social conflict in late nineteenth-century America.

IRWIN YELLOWITZ
City University of New York

MARGARET WALSH. *The Rise of the Midwestern Meat Packing Industry*. Lexington: University Press of Kentucky. 1982. Pp. x, 182. \$14.50.

In this tightly organized, closely reasoned, compact monograph, Margaret Walsh proves once again that

she has more to say than do many in the historical guild. Research, narrative, and interpretative synthesis are remarkably in tune, creating a model of integrative structure. Walsh notes that too often histories of the processing of lumber, hides, and meat have been sterile, isolating those industries as factors in the development of the business firm. She sets out to revise this straight-jacketed deduction and does so completely and successfully.

Affected by seasonal change, the merchant-capitalist, rural meat packer (oriented toward hogs because of their independence, ease of breeding, and availability) was at the mercy of winter temperatures, his ability to command credit, and the always mercurial market. Change arrived with the advent of the railroad, but not with the precipitousness that we have been led to believe. Walsh contends that the challenge of the rail ebbed and flowed for over a decade. Initially, railroad centers such as Indianapolis and Cincinnati profited from the movement of hinterland hogs to metropolitan packing plants. They soon discovered, however, that the railroad was a double-edged weapon, for the rural merchant could now take advantage of the proximity of an Eastern market. For the small meat packer, the verdict was as gradual as it was inexorable; the speed of the railroad in the 1850s destroyed his local advantage and gave the future to his urban competitor. This metropolitan entrepreneur had more capital, easier access to a stable market, and above all, the advantage of a nearby stockyard.

Two decades later, in the 1870s, the urban packer was poised for an economic takeoff, replete with stockyards, refrigerated cars, and a broad financial base. His rural counterpart, facing economic liabilities, eventually turned to specialty meat packing if he survived at all. By the 1890s the meat packing industry had been transformed into an oligopoly.

As with many historical studies the conclusions appear transparent, the decoding inevitable—a testimony, of course, to Walsh’s competence. She has offered a solid historical account and a revision of previous works. It is difficult to imagine such an intricate, complex narrative, compressed so brilliantly into less than one hundred pages (excluding appendixes, notes, bibliography, and index).

GENE M. GRESSLEY
University of Wyoming

DIRK HOERDER, editor. *American Labor and Immigrant History, 1877–1920s: Recent European Research*. (Working Class in American History.) Urbana: University of Illinois Press. 1983. Pp. ix, 286. \$28.95.

The essays in this volume were initially presented in November 1978, at a conference held at the University of Bremen to take stock of European scholar-

ship in American labor and immigration history. Edited for publication by the conference's organizer, Dirk Hoerder, they represent what David Brody and Herbert G. Gutman in their preface call "a first sign of an emergent European school of American working-class history." As Hoerder notes, these essays do not add up to an ideal, interdisciplinary history of the working class. They are, however, nevertheless valuable as "stepping stones" toward such a goal. Significant, too, is that their authors represent nine different nations.

The studies are grouped in three parts, under the headings, "Workers, Scholars, Militancy," "Organization," and "Immigration." Within each part there is much comparative history. Essays in part 1, for example, enable the reader to compare the impact of personal experience on the scholarship of such figures as W. E. B. Du Bois and John R. Commons. A third study compares the standard of living of American workers in Pittsburgh with their counterparts in Birmingham, England. In parts 2 and 3 there is an abundance of material on "Old World" influences on immigrants from Germany, Ireland, Finland, and Yugoslavia.

Some of the essays tackle continuing controversies. In analyzing the failures of the Socialist Labor party in the 1890s, Hubert Perrier resists the temptation to blame the leadership of Daniel De Leon. Instead he presents an interesting thesis, which requires additional elaboration, attributing the problems to declining German and increasing Jewish participation in the party. Andrew Dawson answers the question of why a viable labor party failed to form in the United States. Focusing on the skilled workers, he explains how their relative contentment with their status fostered a craft, rather than class, consciousness and thus prevented political militancy from taking root.

In considering individual immigrant groups, the essays reveal disunity and division. David N. Doyle studies the reception of late nineteenth-century "unestablished" Irish by "established" Irish-Americans. Reminiscent of scholarship on other ethnic groups, Doyle's work demonstrates that the oldtimers were not necessarily eager or even able to assist the newcomers. Two essays examine the Swedes. Lars-Göran Tedebrand disputes the assumption that Swedish-Americans were usually conservative. He cites several radical and reform movements that influenced them and in which they were prominent, and discloses a heritage of political and trade union activity in Sweden. On the other hand, Sune Åkerman and Hans Norman find Swedish workers in Worcester, Massachusetts politically inactive in large part because they had been warmly welcomed into the community by "Yankee Republicans."

The level of scholarship in this anthology is uniformly high. Whether Ferruccio Gambino is dis-

cussing Du Bois's *Black Reconstruction* or Marianne Debouzy is relating the achievements of rank-and-file workers during the railway strikes of 1877, the result is first-rate. European scholars have entered the field of American labor and immigration history with apparent zest. It is an extremely positive development that deserves continued encouragement.

ROBERT D. PARMET

York College,

City University of New York

BALLARD C. CAMPBELL. *Representative Democracy: Public Policy and Midwestern Legislatures in the Late Nineteenth Century*. Cambridge, Mass.: Harvard University Press. 1980. Pp. xi, 260. \$20.00.

This excellent monograph deals with the making of public policy, "the authoritative rules that government sets for society" (p. vi), in five sessions of the legislatures of Illinois, Wisconsin, and Iowa from the mid-1880s into the mid-1890s, focusing in particular on their houses of representatives. In the decade under examination, Republican dominance in the three states was first challenged by the Democrats, most successfully in Wisconsin and Illinois, then was strongly reasserted. During this period the American political system was decentralized and state governments, especially the legislatures, played the largest (albeit limited) role in governing the country.

Representative Democracy rests on impressive research. To study the legislatures of the late nineteenth-century Midwest in their political context, Ballard C. Campbell analyzed some one thousand legislative roll call votes, compiled collective biographies of legislators and profiles of legislative districts, and mined legislative publications, newspapers, and the limited body of relevant political correspondence that survives. An appendix on sources and methods provides readers with valuable information regarding the author's research strategy. Two dozen tables contribute to the presentation.

Campbell first discusses the politics of legislative elections in Illinois (which alone of all states provided for election by cumulative voting in multi-member districts), Iowa, and Wisconsin; the legislators (most of whom served only one term); and the state legislatures themselves. He then turns to the subjects that lie at the heart of his study. He defines "contested issues" and assigns each such issue to one of five general policy categories—community mores, commerce, fiscal policy, government, and public services—which he subdivides into seventeen more specific policy topics. Successive chapters cover party voting in the legislatures, legislation dealing with community mores, ethnicity and dimensions of legislative voting, economic policy, and partisanship

and governmental issues. A final chapter draws together the strands of analysis that run through the book and briefly contrasts the polity of the late nineteenth century with that of our own period.

The public policy making that emerges from Campbell's analysis clearly did not involve the promotion of sweeping, integrated programs, the recruitment of legislators, or the mobilization of voters sympathetic to those programs. But policy making did reveal that legislators and voters of each major political party shared "an informal commonality of interests and attitudes" (p. 97) that contributed to party and constituency-oriented legislative voting on issues relative to community mores. Although the Democrats' antistatism could sometimes be observed in their reaction to other proposals, they lacked party cohesion with regard to the range of economic policy issues that came before the legislatures. Rather, a variety of factors influenced legislators regarding such matters.

Campbell not only rescues late nineteenth-century state legislatures from obscurity, but his clear, solid, and thoughtful study of legislative behavior and policy making also contributes much to our understanding of the politics and society of that period.

SAMUEL T. MCSEVENEY
Vanderbilt University

DAVID TYACK and ELISABETH HANSOT. *Managers of Virtue: Public School Leadership in America, 1820–1980*. New York: Basic Books. 1982. Pp. vii, 312. \$17.95.

WAYNE E. FULLER. *The Old Country School: The Story of Rural Education in the Middle West*. Chicago: University of Chicago Press. 1982. Pp. ix, 302. \$22.50.

Managers of Virtue is, as David Tyack and Elisabeth Hansot subtitle it, a history of "Public School Leadership in America, 1820–1980." The first two parts of the book discuss educational leadership in the common school and the Progressive eras; the last section, regrettably only half as long as either of the first two, attempts to bring the picture up to date by examining the decline in practical and moral authority and the erosion of confidence in school leadership in the last thirty years.

There is much to be grateful for in the first two sections. The authors go over well-trod ground but do so without belaboring the obvious. Their attempt to be fair to their protagonists is commendable. In doing so, they are able to present new insights into the work of educators usually treated in a one-dimensional manner. There is, in these first two sections, an outstanding analysis of the common school movement as a Protestant-Republican cru-

sade carried on by multipurpose reformers convinced of the God-given sanctity of capitalism, evangelical Protestantism, and Americanism; a portrait of the ways in which the educational progressives, through their sponsorship of graduate students, extended their control over big city school systems; and a thorough (if at times exaggerated) discussion of the power of the professionalism ethos. Only in the third part of their work do Hansot and Tyack's attempts at melding the social and political history of the schools break down. The density and subtlety of analysis that marks the earlier sections is sorely needed in the last section on contemporary educational leaders.

There are problems inherent in the project. By focusing on the leaders—to the point of including a number of biographical sketches of exemplary ones—the authors must end by de-emphasizing social and economic forces as a determining factor in the shaping of the programs and policies pursued by those leaders. Although Hansot and Tyack point out more than once the important roles played by businessmen and their associations in the shaping of schooling policies, they never explore in detail the relationship between the "practical men" (Cubberley's term for businessmen) and the educational leaders. One wants to know more about the ways in which the two "elites" worked together, influenced one another, and sorted out their differing priorities.

Wayne E. Fuller's book on rural education in the Middle West in the late nineteenth and early twentieth centuries provides a useful counterpart to Hansot and Tyack's work. The latter tells the story of educational leaders who are for the most part successful in wresting decision-making power from local communities and elites. Fuller's work presents a very different story. His is a portrait of democracy at work on the middle border, of farmers who controlled their own one-room schoolhouses and were successful time and again in resisting the antidemocratic, centralizing thrust of the professional educators.

This is a passionate book. Fuller writes beautifully, and at times movingly, of the one-room schoolhouses, their teachers, parents, school boards, and students. Although one fears that he occasionally overstates his case, especially when he attempts to demonstrate the ways in which the schools were "invaluable laboratories of democracy" (p. 45), one is convinced in the end that his interpretation is correct and his passion in defending local farmer/educators against the professionals admirable. He points out that these rural schools were doing precisely what they were supposed to be doing: they were teaching their students (no matter what their age, background, ethnicity, or constancy in the classroom) the "common branches" with

emphasis on reading, arithmetic, writing, and spelling.

Fuller's book is an excellent corrective to traditional views of rural backwardness in education. It is also a powerful commentary on the irrelevance of school organization vis-à-vis student achievement. For all the polemics against the one-room schoolhouse and in favor of centralization, modernization, and efficient school leadership, Fuller can find no evidence that the consolidated, centralized schools did their educational task better than the one-room schoolhouses. In fact, the opposite might have been the case. Smaller schools and fewer students, contrary to everything the professionals argued at the time, might have promoted learning rather than hindered it. *The Old Country School* has much to say, not only about rural schooling at the turn of this century, but about the larger educational issues that have informed the debate about the quality of public schooling for the past one hundred years.

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PAUL STARR. *The Social Transformation of American Medicine*. New York: Basic Books. 1982. Pp. xiv, 514. \$24.95.

In this two-book volume, Paul Starr analyzes the rise of medical professional sovereignty in the nineteenth-century United States and the transformation of medicine into an industry in the twentieth century, a monumental task that few would be brave enough to try. He has succeeded admirably in making sense out of huge amounts of primary data and secondary literature and in helping us understand how medicine developed from an economically vulnerable group of individuals into an economically advantaged, cohesive profession. The scope of the project, let me emphasize, is impressive in itself: no medical historian to date has provided such a thematic and synthetic organization of the history of the profession. The fresh insights Starr brings to the task are also impressive and valuable. He can take a familiar point, usually glossed over by others, and extract from it new and important meanings. Starr's discussion of the nineteenth-century market for medical services, for example, begins with the known facts of the low level of real income and the geography of rural life, both of which prevented most people from seeking medical help. He expands this into an analysis of direct and indirect medical costs and explains the expanding market later in the century in terms of the decrease in the indirect costs of seeking professional help, aided by the telephone and the automobile. From the partic-

ular he builds a structure for understanding both the past and the present.

The first book, in which Starr analyzes the rise of medical authority in the United States, will be the most valuable to historians. The second book, which develops the theme of twentieth-century corporate medicine and points to the future, will be of most interest to sociologists and economists. Of course, both together should be read by all scholars and general readers interested in the medical profession.

The major issue of book one is the process by which the medical profession gained its status and what Starr calls its "cultural authority" in the period before medical science had much of a therapeutic contribution to make. Starr dates the rising status to the late nineteenth century, when historians have generally agreed doctors rose in the estimation of their contemporaries. This is the period after germ theory had been developed but before it was able to have a significant clinical impact. In order to explain how doctors emerged victorious before they were particularly effective, Starr follows the story from the early nineteenth century when regular practitioners were besieged by competing and successful sectarian practitioners, especially Thomsonians, homeopaths, and eclectics. The regulars' efforts to discredit their opponents led ultimately to an accommodation with them. Starr links these events within medical practice to events outside of it, most significantly the economy and political interests. He emphasizes how the profession accommodated itself not only to dissenters, but also to economic limitations and expansions and to political ideologies. One theme of both books is that the profession was rarely able to get its way totally, yet it was increasingly strong enough to delay unwanted events and to force concessions. One of the best examples of this was medical licensing, an issue that was resolved at the end of the nineteenth century by incorporating the educated sectarians on licensing boards or by creating separate boards for these groups. Ideally, the medical profession would have preferred licensing to exclude the sects; but the profession accommodated itself so that both could benefit from the distinction between themselves and quacks and uneducated practitioners. In similar fashion, the profession accommodated itself to or modified private health insurance plans, medical aid to welfare recipients, hospital organization, women in the profession, group practice, and ultimately corporate and government influence.

The theme of most importance that Starr carries forward in both books is the interrelationship between the medical profession's expanding scientific knowledge and abilities and the political and economic context in which these advances occurred. In order to understand the ascendancy of the profes-

sion, Starr argues, scholars need to look beyond internal medical developments. In the early nineteenth century, Jacksonian democracy created the atmosphere in which elitism could not survive and the multiplication of a pluralist system was welcome. At the end of the century, however, the Progressive ideology, also democratic in intent, put higher value on expertise and hence was able to reinforce the growing authority of science and medicine. The political acceptance of creating a distance between the informed and the uninformed, between doctor and patient, along with the expanding economy that allowed many people their first opportunity to seek advice outside traditional networks, worked together with burgeoning scientific knowledge to increase the medical profession's value and status in the community at large.

Starr suggests that public hygiene and surgery, the two areas in which the new science of bacteriology had the earliest impact, illustrate the power of the new advances in the late nineteenth century. Certainly the promise of surgery without the usual accompanying infection and death played on the public imagination and created its own ethos of the expertise of medicine. But Starr may carry the image of bacteriology's promise too far when he includes public hygiene as both cause and beneficiary of the new prestige of medicine at the end of the century. Starr claims that germ theory and its consequent narrowing of the focus of public health to chasing particular bacteria provided "the basis for an accommodation between public health and business interests" (p. 189) and made public health "more politically acceptable" (p. 191), but the fact is that business and political groups supported garbage collection and disposal and water and sewer systems before bacteriology taught them why these were helpful. Starr leans too heavily on some of the leaders of public health, like Providence's Charles V. Chapin, who were personally transformed by the promises of the new germ theory, and he does not pay enough attention to the actual work of public health departments, which incorporated the new scientific knowledge very slowly and which were not the beneficiaries of particularly high prestige or financial rewards at the turn of the century. A broader time scope would have lessened Starr's emphasis on the role of science in this particular example and led him to follow more closely his own prescription for integrating scientific influences within cultural and political contexts over time.

However many other quibbles I could choose to mention, and there are surprisingly few in a work of this scope, I have enormous admiration for this book. It will be invaluable for students and for scholars who want a systematic introduction to the burgeoning fields of medical and professional history. It will reward readers with its breadth and

depth and provide many avenues for future discussions and research. Starr's insights and analyses will shape American medical historiography for years to come.

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JEROLD S. AUERBACH. *Justice without Law? Resolving Disputes without Lawyers*. New York: Oxford University Press. 1983. Pp. xiii, 182. \$16.95.

"Law begins where community ends," explains Jerold S. Auerbach at the outset of his essay on the relationship between the two in American history (p. 5). Americans have made law their "national religion" (p. 9) for in a society as pluralistic as the United States today law is the principal common denominator. It has not always been so, and Auerbach has hunted and pecked through the last three centuries to find American communities cohesive enough religiously, ethnically, or economically to preserve order without resort to lawyers and courts. The first half of this book discusses these groups, the second half deals with the rise of legal formalism in the last one hundred years. This slim volume introduces these subjects but does not treat them exhaustively (p. 16).

In the seventeenth century Dutch settlers, Puritans, and Quakers used their demanding religious and moral principles to resolve disputes. "For them law was a necessary evil or a last resort, not a preferred choice" (p. 20). In the early nineteenth century Christian utopian communities, including the Shakers and Mormons, rejected litigation "as the private equivalent of violence and war" (p. 51), relying instead on the group's shared religious and moral sentiments. In the late nineteenth century certain Scandinavian, Chinese, and Jewish immigrants rejected Western legal values in favor of Old World dispute settlement practices. In none of these communities did alternative forms of dispute settlement survive much beyond the first generation. With the exception of certain immigrant enclaves and commercial associations, no American communities were cohesive enough after the middle of the nineteenth century to avoid the rule of formal law.

In the twentieth century mediation has survived mainly as a weapon in the arsenal of the powers that be. "Until the Civil War alternative dispute settlement expressed an ideology of community justice. Thereafter, as it collapsed into an argument for judicial efficiency, it became an external instrument of social control" (p. 57). So, in labor disputes arbitration was promoted by both judiciary and management to create the illusion of fair dealing. Small claims courts were used to divert the poor into

an informal process where their rights to due process were violated in the name of efficient justice. "In practice (whether or not by design)," alternative dispute settlement "was most enthusiastically prescribed for disadvantaged citizens who only recently had begun to litigate successfully to protect and extend their rights" (p. 124).

Auerbach does not like the legal establishment, because he feels the haves use it to oppress the have-nots. That is why he quit law school twenty-five years ago (p. 175). It is important to keep that in mind when reading this self-described "interpretive analysis" (p. 16). He asserts, for example, that "A professional community of lawyers and judges has wrenched mediation and arbitration from local communities that once resisted law as an alien value system" (p. 15) to maintain the supremacy of a "predominantly white, secular, capitalist culture" (p. 67), which he sees as a "Darwinian jungle" (p. 141).

Taken as questions, Auerbach's assertions suggest the need for closer examination of the historical relationship between formal and informal dispute settlement in American life. In this respect, his provocative essay is an important contribution.

ROBERT SILVERMAN
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GEORGE ERNEST WEBB. *Tree Rings and Telescopes: The Scientific Career of A. E. Douglass*. Tucson: University of Arizona Press. 1983. Pp. xiii, 242. \$19.50.

The history of the sciences in the United States still suffers from a lack of elementary, essential information on individuals, institutions, events, and concepts. George Ernest Webb's biography of an astronomer, whose greatest contribution was the founding of dendochronology (the use of tree rings for historical datings), is a welcome addition filling a significant gap. A straightforward biographical account based in large measure on pertinent manuscript sources, Webb's book opens a number of interesting problems for future study. Andrew Ellicott Douglass (1867–1962) was not a major figure but his work is neglected simply because it is largely outside the main concerns of various specialties like the history of science and the history of ideas.

By far the best part of the book is on the tree-dating work. But even here Webb displays a reluctance all too present in the other sections. There is very little in the way of posing of interesting historical questions, of getting into an analytical mode. Douglass was not a typical astronomer, even disregarding his interest in tree-dating. But he was by no means unique among astronomers in a concern for meteorological problems, the source of his work

with trees. There is an interesting intellectual tradition here not unrelated to the geographic determinism now reviving in some French intellectual quarters.

By now we are accustomed to the use of carbon-14 dating and other physical methods in archaeology. When Clark Wissler approached Douglass in 1914, their meeting represented an early but not unique contact between differing intellectual modes. (A whole barrage of dissertations are implicit in the relationship of anthropology and the physical sciences.) Douglass's work on dating sites in the Southwest was part of a developing sense of region. Arguably, dendochronology was less likely to arise in a humid region. The dating of pueblos and archeological sites occurred as the Southwest became self-conscious of its roots, its contemporary deficiencies, and its aspirations. Here the book advances in a very interesting way but Webb is too tentative. It does not help that the account of Douglass's public life outside of scientific research from 1901 to 1930 is segregated into a chapter breaking up the chronology of his intellectual career.

Despite these reservations, I certainly learned from Webb's biography and suspect many others will also. This kind of study enlarges our vision, and for that we are indebted. Given the author's knowledge and obvious skills, I hope he will follow this revision of his thesis with further research.

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SUSAN R. SCHREPFFER. *The Fight to Save the Redwoods: A History of Environmental Reform, 1917–1978*. Madison: University of Wisconsin Press. 1983. Pp. xviii, 338. \$22.50.

The wood of the California coastal redwood (*Sequoia sempervirens*) is one of the most valuable of timbers, especially for exterior building use. The tree itself is awe-inspiring—mature specimens tower hundreds of feet and date from thousands of years in the past. Coastal redwood forests inspire the romantic and delight the recreationist. To the scientist, the forests are remarkable biological resources, providing a unique opportunity to trace the progress of natural communities over time and including species present nowhere else but the coast of California.

Susan R. Schrepfer, in a masterfully crafted piece of scholarship, focuses her attention on a six-decade struggle to preserve remnants of the coastal redwood forests in their virgin state, from the founding of the Save the Redwoods League in 1918 to the passage of the second Redwoods National Park Act in 1978. All the different groups interested in the great trees—loggers, various types of conservation-

ists, private associations, government agencies, and scientists—play a part in her story.

In basic outline, the Save the Redwoods League spent its first decade in raising private funds and throwing its considerable influence behind the establishment of a state park system in California, resulting in four parks created to protect redwood forests by 1928. The league included in its membership scientists—John C. Merriam principal among them—and high-minded philanthropists of the sort that characterized much of Progressivism. The league was, therefore, suspicious of the National Park Service, which emphasized recreation and development, and inclined to favor state and private efforts at conservation. Its road was not easy, however.

In the 1950s logging and road construction increasingly reduced the dwindling redwood forests, and a newly activist Sierra Club—once a small band of hikers, now a committed corps of middle-class reformers—began to dominate the campaign to save the redwoods, calling for federal intervention. In 1968 they triumphed with a compromise national park that impossibly combined logging and wilderness recreation in the same watershed. A decade later, another compromise—the most expensive land-acquisition measure in American history—enlarged the park and provided unprecedented pay-offs to local industrial and labor interests.

This is not a simple or ordinary history of a conservation crusade. Schrepfer very ably traces the changes in scientific wisdom from nineteenth-century romanticism and teleological evolutionism to more current ecological dynamism—and the influence of those intellectual developments on political history. She also offers trenchant analyses of the population of various reform and interest groups, and along the way offers critical studies of historians' judgments on such things as Progressivism, the New Deal, and the histories of science and conservation.

This is an excellent book, whose few minor errors are inconsequential. If it has a weakness, it is perhaps in some loss of the human element. Although the author does a good job in addressing people in the mass, as organized in various groups, she seldom gives a sense of the personalities of the major characters. But there is so much of value in the book as it is, that this is a minor quibble. The subject is important—much broader than the title suggests—and so is the book.

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SUSAN L. FLADER, editor. *The Great Lakes Forest: An Environmental and Social History*. Minneapolis: University of Minnesota Press or Forest History Society, Santa Cruz, Calif. 1983. Pp. xxxii, 336. \$29.50.

J. RONALD ENGEL. *Sacred Sands: The Struggle for Community in the Indiana Dunes*. Middletown, Conn.: Wesleyan University Press; distributed by Harper & Row, Scranton, Pennsylvania. 1983. Pp. xxii, 352. \$22.95.

These volumes display two varieties of environmental history. The concern of Susan L. Flader and her contributors focuses on an international bioregion a thousand miles wide centered on the Great Lakes. J. Ronald Engel focuses on a fourteen mile strip along one of the lakes. Flader's book marshals the talents of geologists and biologists; her subject is truly the history of an environment. Engel relies on the more familiar tools of the intellectual and political historian to tell the story of how people endeavored to protect a place.

Flader's edited work originated in a 1979 symposium entitled "Environmental Change in the Great Lakes Forest." It brings together not only scientists but geographers and authorities on American literature as well as historians. The intent is to understand a region, past and present, in its full complexity. Flader calls this "an ecosystem conception of history." She is aware of the antecedents of this approach. Frederick Jackson Turner, who, parenthetically, grew up in the Great Lakes forest, took as the central theme of his work the relationship between environmental context and ideas and institutions. Another harbinger was James Malin whose book, *The Grasslands of America* (1967), has received delayed recognition as a pioneer effort to link environmental and human change. Of course Walter Prescott Webb's *The Great Plains* (1931) constitutes a model for any scholarly investigation of land and people. Indeed, with Flader's book as evidence, one cannot help but feel that geography, after something of an eclipse in historical study, is regaining recognition as a primary determinant of human life.

Flader and her colleagues assume at the outset that history neither begins nor ends with human beings. People are part of a biophysical context. How they live and what they think is a product of that context. True, technological civilizations have impressive capacity to modify the environment. But *The Great Lakes Forest* offers many reminders that the natural setting still calls the most important shots. The forest, for example, has its own dynamic. Certain trees grow in certain places in a certain order ("forest succession") without much regard for what people prefer. And no culture, from the mound-building Paleo-Indians to contemporary farmers mounted on John Deere tractors have been able to do much about the northern limit of agriculture. Ice storms, disease, wind throws, and lightning strikes continue to shape the modern forest as they did the historic one. There are, in short, environ-

mental "givens" to which society, for all its technological conceits, is obliged to accommodate. Understanding these constraints may ultimately be more important for the historian than concentrating on how people have modified their world. The Flader book at least implies this perspective.

One contribution of *The Great Lakes Forest* is to question the linear theories common a generation ago in both biology and history. Biologists used to understand forests as an evolving system moving inexorably to a permanent, stable condition called a "climax." This was the "virgin forest" or "forest primeval" that captured the imagination of poets and nature preservationists. The problem is that such forests existed primarily in the imagination. Real forests are mosaics. They change constantly with no stable, balanced "climax" at the end of the ecological rainbow. The life forms, mankind included, that succeed in the forest have also learned to change.

Earlier historians, following Turner, were similarly captivated by a linear view of human evolution. A kind of social succession replaced trapping with lumbering and mining and finally the "climax" condition: urban living and manufacturing. The process defined progress. What it ignored was the fact that some environments and some people were better off stopping short of cities and factories. The best thing much of the Great Lakes forest can do, Flander's book suggests, is to continue being a forest. The land is best suited to growing trees and, as Samuel Hays's essay explains, provide outdoor recreational opportunities for posturban Americans.

Sacred Sands concerns the Indiana Dunes at the southern tip of Lake Michigan. It describes, in impressive detail, how this remnant of naturalness was, almost miraculously, snatched from the urban-industrialism of metropolitan Chicago and Gary. The fact that ten million people live within an hour's drive of the dunes underscores the achievement.

The key dates in nature protection here are 1923, when Indiana established a state park, and 1966, when the federally administered Indiana Dunes National Lakeshore salvaged the remaining bits of undeveloped land. The efforts of Illinois Senator Paul Douglas are an interesting and well-documented part of Engel's presentation of the latter story.

Historians of science will welcome Engels's account of how the Indiana Dunes figured in the emergence of the science of ecology. In an excellent chapter featuring the early twentieth-century studies of Henry Chandler Cowles of the University of Chicago, Engels shows how first the glaciers and then the moving sand created opportunities for scientists to observe ecosystem evolution. Readers familiar with Donald Worster's *Nature's Economy*

(1979) and Susan Flader's earlier book, *Thinking Like A Mountain* (1974), will profit from Engel's addition to our understanding of the birth of the ecological sciences.

Other chapters in *Sacred Sands* explore the meaning of the dunes for Midwestern poets, notably Carl Sandburg, for artists, and for Progressive reformers interested in providing recreational opportunities for slum dwellers. Indeed Engels, who is a professor of theology, strains to give the dunes central importance in America's "civil religion"—democracy. A little of this kind of speculation about "America's origins and destiny" goes a long way. The story *Sacred Sands* tells is impressive enough; for Engel to insist that the Indiana Dunes are the "sacred center of the religion of democracy" is overkill. The book is strongest when it simply tells how a half century of effort and love snatched a small piece of the old Great Lakes landscape from certain transformation.

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ARRELL MORGAN GIBSON. *The Santa Fe and Taos Colonies: Age of the Muses, 1900–1942*. Norman: University of Oklahoma Press. 1983. Pp. xiii, 305. \$24.95.

Arrell Morgan Gibson, with such books as *The Kickapoos* and *The Oklahoma Story* to his credit, has established a new direction. His book might be called, *The Historian Looks at Esthetics*. Undismayed by the unfamiliar terrain, he has turned in a remarkable performance.

His subject is the twin colonies of creative people that grew up almost side by side at Santa Fe and Taos, New Mexico, beginning about 1900 and ending with the onset of World War II. The painters came first. Suffering from "a dark disenchantment with industrialism and machines . . . and a glowering impatience with . . . the enlarging urban mass" (p. 6), they fled to various havens across the country. Santa Fe and Taos "attracted more creative folk during the age of colony building . . . than any other area of the United States" (p. 9) and functioned longer and better.

The remoteness of northern New Mexico was an attraction, but just as important were the mountain climate; the "cobalt-blue sky trimmed with silver-edged cloudlets; brilliant light . . . and adobe *casas*, domiciles for the colorful tri-ethnic cultures" (p. 30).

The first comers were a few good, conventional painters, among them Joseph Sharp, Ernest Blumenschein, and W. Herbert Dunton. By 1912 these pioneers were being pushed aside by a flood of newcomers who brought "new dogma, design, and style" (p. 30). Such painters as Marsden Hartley and

John Sloan found a more congenial artistic climate in Santa Fe, where Carlos Vierra and Kenneth M. Chapman had already established a foothold. By 1920 the two colonies were internationally known.

Writers appeared on the scene early, and by the mid-twenties they had forged ahead of the artists. Poets of the caliber of Carl Sandburg, Robinson Jeffers, and Robert Frost came to visit, and permanent settlers included Witter Bynner, Mabel Dodge Luhan, and Mary Austin. Three chapters near the end of the book are devoted to the personalities, peculiarities, and achievements of Luhan, Austin, and the great English immigrant, D. H. Lawrence.

By 1942, thanks to the Depression, a blazing feud between colonists and townspeople, and the Second World War, the colonies went into eclipse. Nevertheless, they left a "sumptuous legacy" of "taste, style, and sophistication" (p. 272), which still survives.

Anyone interested in Southwestern art, literature, or Indian cultures will have some acquaintance with parts of Gibson's story. His great service is in putting all the parts together. A reader who knows and loves Frank Applegate's *Indian Stories from the Pueblos*, for example, will find that the man was a painter, playwright, teacher, specialist in Indian textiles and pottery, and a civic leader.

There are some signs of hasty editing, perhaps because the publisher skipped galley proofs. There is some faulty Spanish: *sanctuaría* for *santuario* (p. 214); *alabadas* for *alabados* (p. 174); the article "a" with *horno* (p. 234). Word choice is sometimes debatable: "muse," as in "muse cause," "muse residents" (pp. 269, 273); "sculp," "emote," and "rapture" as verbs (pp. 87, 204, 239); "exotic" as applied to Indian art (p. 149). Such minor matters, of course, do not diminish the real value and importance of this well-crafted, scholarly, and useful work.

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JAMES REED. *The Missionary Mind and American East Asia Policy, 1911-1915*. (Harvard East Asian Monographs, number 104.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1983. Pp. xiv, 258. \$20.00.

James Reed analyzes the origin and unfortunate results of the fixed and firm belief that American foreign policy toward China must be an unselfish approach consistent with Christian ethics, what he terms the "Missionary Mind." This extremely readable and provocative study relates less to missionaries than it does to how this attitude came to dominate the thinking of Protestant America and to shape China policy.

The author sees this frame of mind reigning supreme far beyond the years 1911-15. He is convinced that, with few exceptions, China policy has not served national interests, that it ignored the realities in China, that it led to empty rhetoric unsupported by force, and often misled the Chinese. "This lack of clear-sightedness," he writes, "this selective perception and this selective recognition of Asia, can be attributed to the Protestant missionary movement" (p. 3).

Reed has read widely in religious journals, congressional debates, and secular periodicals such as *Outlook*, *New Republic*, and *Independent*. He presents a persuasive case for the dominance of the missionary mind among the public. China became the target for the crusade to extend Christian civilization. What the spokesmen meant by Christian civilization was really Anglo-Saxon civilization, for it was among the Protestants that this ideology fixed its hold. Reed contrasts this with the apathy of American Catholics concerning China. The myth of a "special China" was a hundred years in the making, beginning with the establishment of the American Board of Commissioners for Foreign Missions in 1810 and finally reaching a crest after 1890.

The author sets forth, not as an established fact but as a conjecture, that "small numbers of men, usually wealthy, influenced the opinions of large numbers, drawn from the bureaucratic and professional classes, thereby effectively creating a public consensus to undergird American foreign policy" (p. 81). This foreign policy elite was well informed about Europe, but dismally ignorant of Asia. "There is ample evidence, necessarily general and circumstantial," Reed states, "to suggest that the Missionary Mind rushed in to fill this vacuum" (p. 94). Given the importance of the religious press and the hundreds of missionaries who on home leave spoke in churches throughout the country, Reed's conjecture is well based.

But the friendly interest in China lacked an economic underpinning, for it was Japan and not China that offered the far greater market for goods and opportunities for investment. Business interests responded accordingly, but from the overthrow of the Manchu dynasty in 1911 to Japan's Twenty-One Demands in 1915 government policy was sympathetic to China, at least sufficiently to employ friendly rhetoric whereas it was antagonistic toward Japan.

The reviewer ventures to suggest that while the missionary mind created a strong pro-Chinese feeling, the government did not necessarily respond. Only in the instance of Woodrow Wilson's granting recognition to the republic and refusing to support the consortium of bankers is the evidence clear and sharp that missionary influence really counted. Bishop Bashford and the representatives of the North American Conference of Foreign Missions

who lobbied in Washington were more often looked on as troublesome meddlers. The anti-Japanese feelings that came to the fore in 1915 owed more to the Japanese seizure of the islands in the Central Pacific than to the pro-Chinese pleadings of missionary groups.

PAUL A. VARG
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BERNARD D. COLE. *Gunboats and Marines: The United States Navy in China, 1925–1928*. Newark: University of Delaware Press or Associated University Presses, East Brunswick, N. J. 1983. Pp. 229. \$28.50.

This well-researched book focuses narrowly on the role of the United States Navy's Asiatic fleet in the support of America's China policy during the critical period of that nation's unification struggle under the leadership of the Kuomintang party and its generalissimo, Chiang Kai-shek. During the four years from 1925 to 1928 Chiang's forces defeated in combat or bought off with "silver bullets" the most important warlords who were resisting the Kuomintang's program for national unity. In the process of achieving near mastery over China, the Kuomintang awakened and used a new spirit of nationalism to attempt the revision of the "unequal treaties" system that had held China by the throat since the mid-nineteenth century. Also awakened was an active xenophobia among all classes of the people. By 1925 the Chinese had turned violent in their insistence on termination of tariff controls, extraterritoriality, missionary proselytization guarantees, and the right of foreign gunboats to sail the rivers and lakes of the Yangtze Valley and South China. The United States and all other nations who possessed special treaty rights in China now had to face the problem of defending the lives and properties of their citizens. American policy was complicated by the nation's desire to see China's unification achieved, but without endangering American lives and without collaborating with other nations in defending foreign lives and property.

Bernard D. Cole, a career naval officer with a doctorate in history, concludes that the Asiatic fleet succeeded in its enforcement of American policy. Despite the fact that the Yangtze patrol force and the South China patrol force were assemblages of maritime relics, they did protect American lives without coming into serious conflict with organized Chinese forces except at Nanking in 1927. Although the subordinate patrol force commanders, and the American minister in Peking, urged more vigorous action and direct collaboration with British, Japanese, and French military and naval forces, the Asiatic fleet commanders and the State Department insisted on less provocative measures. Withdrawal of

American nationals from the chaotic interior provinces was the usual approach. Property could be restored or indemnification sought later, but lives could not be replaced. Although the book makes clear that the navy's gunboats were hardly the omnipresent agents of American imperialism that revisionist historians have described, their very presence on China's waters were still an affront to the Chinese.

The story is reasonably well told with a minimum of superfluous verbiage. There is full investigation of public and private manuscripts in the United States. Consular reports and the English language press were used to present the view from China. It was good to see use of the Smedley Butler, Glenn Howell, Mark Bristol, and Nelson T. Johnson papers. On the other hand, since so much was written concerning the American minister to China, John Van Antwerp MacMurray, it is disappointing to find that neither his papers at Princeton University nor published writing about him, particularly by Thomas Buckley, were investigated by the author. One final point, the island and the gunboat are spelled, Tutuila. Overall, I consider this a good first book and "must" reading for those interested in America's Far East policy and its historical development.

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WAYNE S. COLE. *Roosevelt and the Isolationists, 1932–45*. Lincoln: University of Nebraska Press. 1983. Pp. xii, 698. \$26.50.

Roosevelt and the Isolationists is the culmination of Wayne S. Cole's work on the isolationism of the 1930s. It brings together the results of prodigious archival research, correspondence and interviews with a great many of the protagonists, thorough familiarity with a substantial body of secondary literature, and three decades of thought and reflection. No future volume is likely to match it in thoroughness of research or judiciousness of interpretations, and in that sense it is surely a definitive study.

In thirty-three succinct and eminently readable chapters, Cole provides a history of the Roosevelt presidency with a focus on foreign policy and of the isolationist position during the years of that presidency. He skillfully interweaves internal and external developments to demonstrate the "uneasy alliance" between Roosevelt and the isolationists during FDR's first term, their "parting of the ways" during the second, and the president's final triumph, which came in 1941 and left the isolationists thoroughly vanquished.

The outlines of Cole's analysis are familiar, in part

because of his own excellent earlier work. What he has added is a plethora of often fascinating details that enhances the story enormously. We once again encounter Roosevelt, the masterful politician and quondam internationalist who, given the internal misery caused by the Depression, concentrates on domestic concerns and needs the votes of the isolationists to support his relief and recovery programs. We see these votes increasingly withheld, particularly after the court-packing controversy, at the same time that events outside America's borders become more threatening. As war approaches and then comes to Europe, we observe the danger of war becoming a reality for the United States and the increasing alienation of the isolationists from the president and his policy. And finally we view, perhaps more precisely and more vividly than before, how the Pearl Harbor attack put an end to an already nearly moribund isolationism. Clearly, the global power that emerged from World War II was very different from the country that had slipped into the Depression fifteen years earlier. A missing ingredient was the powerful isolationist element that socioeconomic development, world events, Roosevelt's leadership, or some combination of all three had largely eliminated.

The catalyst for Cole was "Roosevelt and his administration" (p. 365), and it is the president who emerges as the central personage in the study. Not only is he the winner of the contest with the isolationists, he represents the forces of modernity in the struggle with traditions that are being overtaken by events. "Franklin D. Roosevelt," Cole concludes, not necessarily with approval, "was more nearly in tune with what the United States and the world were becoming" (p. 556). But Cole's heart remains with the Western Progressive Republicans who, as in his earlier books on Nye and Lindbergh, are the "true" isolationists whose agrarian values and rural traditions are overpowered in the modern world. To be sure, non-Progressives like Arthur Vandenberg and non-Republicans like Burton K. Wheeler are acknowledged as the prominent isolationist spokesmen they were, but others even further beyond the pale, like David Walsh of Massachusetts, Herman B. Kopplemann of Connecticut, Maury Maverick of Texas, and the whole group of socialist isolationists are given short shrift.

By focusing largely on the Western Progressive Republicans Cole gives consistency to his analysis, but limits his examination of the full complexity of the isolationism of the thirties and of its demise. Only Western Progressive isolationists really moved from "uneasy alliance" to ultimate annihilation. The others were either Roosevelt opponents on both domestic and foreign policy from the beginning or, regardless of their starting position, eventually came to agree with the president's foreign policy views. It

is at least arguable, however, that they were also "true" isolationists.

Although Cole may thus not have said the last word on the subject, *Roosevelt and the Isolationists* has a great deal to offer both to historians and to the general reader. It shows a breadth of knowledge and depth of understanding not frequently encountered and richly deserves a large audience.

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WILLIAM P. HEAD. *America's China Sojourn: America's Foreign Policy and Its Effects on Sino-American Relations, 1942-1948*. Lanham, Md.: University Press of America. 1983. Pp. xii, 353. Cloth \$25.75, paper \$14.25.

This book is a dissertation that should never have come into print in its present form. Its pages display the eagerness of the graduate student rather than the insights of the maturing scholar. The book does not deepen our understanding of American China policy because William P. Head's purpose is unclear, his organization of materials poor, and his research incomplete and outdated.

Given the enormous literature on Sino-American relations in the 1940s, anyone publishing on the subject four decades later should have a clear, unique purpose. Head does not. In a single volume he would explain the origins of the Cold War in China, examine American perceptions of China, and evaluate the impact of Washington's policies on bilateral and global international relations (pp. ix-x). He cannot succeed in the first goal because he rules out consideration of Chinese and Soviet policies. The second theme he treats in a shallow, timeworn manner. While contrasting the thoughts of foreign service offices with the fulminations of Ambassador Patrick Hurley, he fails to examine the perceptions of others who shaped China policy in any systematic way. Nothing is said about the views of the Joint Chiefs of Staff; nor does Head reconstruct how Franklin D. Roosevelt or Harry Truman saw China. He asserts that the American people were "never really . . . made aware of the true situation in China" (p. 250), without so much as a word about their perceptions as revealed in polls, popular journal articles, and editorials and letters to the editor in major metropolitan dailies. Head cannot achieve his third goal because he does not explain what policy, if any, Washington developed and followed. Instead, he laments the results of Washington's actions, wistfully hinting that somehow the past thirty years might have been very different.

Head organizes his material in chronological fash-

ion. After tracing the arrival of various American diplomats, intelligence agents, and military officers in China, he describes their quarrels—most notably that between Ambassador Hurley and his staff. He summarizes the Marshall mission, then concludes with passage of the China Aid Act of 1948. Never does the reader learn why the study should begin in 1942 and end six years later. Individual chapters lack thematic unity. Those for 1947 and 1948, rather than analyzing the fierce bureaucratic differences over China policy, simply recount the heroic version of the origins of the Marshall Plan and its China aid appendage.

The author also fails in his purposes because he simply has not made thoughtful use of the archival and secondary sources available. Head is perhaps at his best in using the papers of wartime military and intelligence figures found at Stanford's Hoover Institution. He relies with surprising carelessness on what foreign service officers told him in interviews; not once, for example, does he check or supplement what they told him by referring to the huge volume of unpublished China reportage in the files of the Department of State. Specialists will also note a disturbing pattern in his citation of secondary works. While Head lists a few books published since 1978 in his bibliography, their findings do not inform his text. General "Vinegar Joe" Stilwell is relieved without the benefit of Robert Dallek's analysis in *Franklin D. Roosevelt and American Foreign Policy, 1932–1945* (1979). The latter half of the book ignores Ernest May's *The Truman Administration and China* (1975).

The result is neither a new monograph nor a fresh synthesis of what Herbert Feis thirty years ago called the China tangle. The time has come for a fresh approach to the study of Sino-American relations in the 1940s. We have the facts. We have outlived the euphoria and might-have-beens of the normalization era. We have excellent specialized studies in Dorothy Borg and Waldo Heinrichs's *Uncertain Years* (1982). What we now need is a much more sophisticated, systematic analysis of how Americans dealt with China in war and revolution. We should try to explain why presidents were so often ignorant and their advisors so often enmeshed in bureaucratic warfare when dealing with Peking. We might even try analyzing British and American China policies in tandem; the comparison might well reveal systemic flaws in the ways Americans and Chinese try to deal with one another.

ROGER DINGMAN

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PETER HERDE. *Pearl Harbor, 7. Dezember 1941: Der Ausbruch des Krieges zwischen Japan und den Vereinigten*

Staaten und die Ausweitung des europäischen Krieges zum Zweiten Weltkrieg. (Impulse der Forschung, number 33.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1980. Pp. xxi, 582.

A professor of history at the University of Würzburg, Peter Herde has written the first major German work on the origins of the Pacific War to incorporate recent revelations on prewar and wartime code-breaking, as well as contemporary theories of bureaucratic politics and behavioral science. The study rests largely on published materials, including works in German, English, and Italian. Primary sources come mainly from published materials as well—*Foreign Relations of the United States*, *Documents on German Foreign Policy*, and the *Hearings of the International Military Tribunal, Far East*, although there is significant documentation from the Public Record Office and, as one might expect, from German political and military archives.

The author advances a point embodied in much Japanese scholarship on the coming of the war, namely, that the Pacific conflict was the regrettable result of Eastern and Western mentalities failing to mesh, of a sad mutual misunderstanding, evident in fateful misperceptions at every milestone on the road to war. Herde maintains that it is a mistake to regard Japan's policy in 1941 as a great deceptive maneuver in preparation for attack on the United States. He rejects any theory of a Japanese-German conspiracy to destroy American power in the Pacific, and together to rule the world, or even East Asia. Moreover, he contends that the United States could have, and should have, played for time. It was neither necessary nor useful, he believes, to have forced Japan's hand. If the United States had been less precipitate, German failures in Russia, Japan's own urgent need for raw materials, and a turning of Japanese domestic politics against the radical military faction would have produced a better, less dangerous East Asian situation by late 1942, and the Japanese would not have attacked southward and to the east.

Herde makes two special points in regard to "Magic." Before Pearl Harbor, he notes, the people who deciphered Japanese codes were regarded merely as technicians, not as people to whom to turn for political-military or strategic analysis. He also points out that the American lack of an espionage system within Japan limited the usefulness of what was obtained through code-breaking. The United States did not know, for example, as did the Germans and perhaps the Russians, that a Japanese decision in principle to attack Southeast Asia rather than the Soviet Union had come already in July 1941.

In passing, Herde pointedly criticizes American treatment of Japanese in Hawaii and on the West

Coast, repeating now familiar charges of racism, dirty politics, logrolling, and the like. He remarks the postattack hysteria of both military and civil officials in the Hawaiian Islands as one of the saddest aspects of Pearl Harbor, and regards the extent and duration of martial law in the islands as excessive. American military leadership, preparedness, and intelligence all appear in rather unflattering terms, to say the least. For a more useful grasp of these latter, one must certainly turn to other works, such as Jeffrey Dorwart's recent *Conflict of Duty: The U.S. Navy's Intelligence Dilemma, 1919–1945* (1983).

In sum, this book is a workmanlike, detailed synthesis of relatively well-known materials, with many of the marks of better German scholarship: excellent setting of context, careful narrative, copious and clear documentation, extensive bibliography. It will surely rank as the leading German-language work on this topic for a long time to come, although its length, level of detail, and modest interpretive contribution make it an unlikely candidate for translation and wider circulation.

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ROBERT DALLEK. *The American Style of Foreign Policy: Cultural Politics and Foreign Affairs*. (Borzoi.) New York: Alfred A. Knopf. 1983. Pp. xx, 313. \$16.95.

Inspired by the writings of his mentor, Richard Hofstadter, Robert Dallek has set out to examine the undue influence of domestic concerns and attitudes on the making of twentieth-century American foreign policy—undercurrents of mood or tone, “a climate of feeling that almost imperceptibly insinuates itself into concrete ideas and actions” (p. xiii). Before 1945, he feels, rapid changes at home made United States actions abroad erratic; since 1945 Americans’ bewilderment and discontent with their new conditions have made them long for old ideals of individualism and encouraged an impulse toward destructiveness in international relations. At the same time distant external threats, especially Russian communism, have become “a convenient excuse for not facing up to troubling domestic concerns” (p. xviii).

One may easily agree with Dallek that American domestic concerns and attitudes have too often been neglected by diplomatic historians. Still, his thesis poses problems that may well be beyond solution. How large a part of the people held these active concerns and attitudes? How and how well did they make them known to the government? How much attention did policy makers actually pay to them (as distinguished from what the policy makers said or

wrote)? How did the subordinate bureaucracy affect decisions and their execution? Why did apparently similar domestic conditions exert markedly different effects on foreign relations at different stages of American social development?

It is not surprising that the rest of Dallek's book only occasionally lives up to its introduction. Instead of a penetrating analysis of American social change and its direct effects on foreign policy, he has usually had to settle for policy makers' imperfect perception of social changes and public desires. The most successful chapters cover the period before 1914. Here Dallek, building directly on Hofstadter's foundations, attributes American imperialistic ventures to a partly unconscious effort to restore self-confidence after the woes of the 1890s, heal the divisiveness caused by the wrenching changes of industrialization, and vindicate the individualism of small-town America. Some will object to Dallek's oversimplification, but at least he establishes a direct line from popular feeling through national leadership to foreign policy.

After World War I this direct line becomes more and more difficult to discern. Dallek explains many policy moves of the 1920s in terms of universalist pacifism but does not reconcile it with the rampant economic nationalism of the decade. His treatment of isolationism in the 1930s is only partly convincing, and, most surprising of all, he has little to say about the McCarthyite hysteria of the 1950s, surely a prime case study for his thesis. Beginning with the Truman administration, the book rarely goes beyond orthodox presentation of policy decisions and campaigns, explained mainly in terms of international conditions, policy makers' personalities, and their perceptions of the popular temper. For example, Dallek attributes Lyndon Johnson's lies about the Dominican intervention of 1965 to fear of Caribbean instability, desire to avoid giving the Republicans an impression of hesitancy, and an effort to prove that the United States could not return to the “simpler, more autonomous world” described by Barry Goldwater in his 1964 campaign (p. 242). Most historians might accept the first two but not the third—certainly not without more evidence than Dallek offers about the thought processes of Johnson and the American people.

In effect Dallek has attempted an ambitious project of individual and collective psychohistory. Such a project ought to be based on exhaustive study of the lives and thinking of every important American policy maker of the twentieth century and also of the social and psychological development of the American people during the same period. Instead Dallek has relied almost wholly on conventional diplomatic monographs and period surveys, a few memoirs, and an occasional broader study with sociological or psychological overtones by such writ-

ers as Robert E. Osgood and David Riesman. This is simply not enough.

DAVID M. PLETCHER
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DANIEL M. JOHNSON and REX R. CAMPBELL. *Black Migration in America: A Social Demographic History*. (Studies in Social and Economic Demography.) Durham, N.C.: Duke University Press. 1981. Pp. vii, 190. Cloth \$16.95, paper \$8.95.

The history of black migration is inextricably linked to the major themes of black American history—the development of slavery, the impact of the Civil War and Reconstruction, the development of Jim Crowism in the late nineteenth century, and the civil rights and nationalist movements of the twentieth century. To write a general history of black migration, therefore, is to describe virtually the entire black experience in America. This formidable task had not previously been undertaken since Carter G. Woodson published *A Century of Negro Migration* in 1918. Now, Daniel M. Johnson and Rex R. Campbell attempt to synthesize the vast amount of relevant material in less than two hundred pages; it is not surprising that they are unsuccessful.

To their credit, Johnson and Campbell, both sociologists, realize that the causes and consequences of migration can be understood only within the context of broader historical currents. Hence, in each of their ten chronological chapters, they begin with a brief summary of the history of the period. It is understandable here that they should rely on secondary sources, but there is no excuse for using outdated and questionable sources. Their footnotes on slavery—one of the most dynamic fields in American historiography—include only two works published in the last twenty years, and their discussion of urban race riots, the subject of an extensive monographic literature in recent years, is based largely on textbook accounts. The result is superficial history with oversimplifications of complex issues, and even occasional factual errors. For example, the Chicago *Inter-Ocean*, a newspaper of general circulation, is identified as a black publication (p. 52).

More disappointing is the unimaginative presentation of the demographic material. Each chapter is subdivided into sections on the causes of the migration, its volume and duration, characteristics of the migrants, and the consequences of the migration. Since many of these factors remain constant over time, there is much repetition. For the scholar or student of black American history, there is little here that is new. The statistics are based largely on published census reports that have long been familiar to historians. It is useful to have these statistics

together in a convenient format, but the accompanying text does little more than describe in words what the tables indicate clearly in numbers. There are only two charts and—even more surprising in a book on migration—only three maps, all of which convey the most basic kind of information. In an era when even nonquantitative historians have become accustomed to sophisticated statistical methodology, one expects more from professional demographers.

Ironically, the most tantalizing part of this book is the epilogue—which is exactly one page long. There we are told that in the late 1960s and early 1970s the long-term pattern of black migration from South to North began to ebb while the movement from North to South increased. This dramatic reversal is the subject of Campbell's current research. Here is a fresh and important subject about which we can learn much from social demographers. The results of this research should be eagerly anticipated.

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ROBERT WEISBROT. *Father Divine and the Struggle for Racial Equality*. (Blacks in the New World.) Urbana: University of Illinois Press. 1983. Pp. 241. \$17.50.

Hallelujah! Father Divine and his Peace Mission, after nearly a half century of scholarly neglect or caricature, are now the subjects of an accurate, eloquent, and judicious full-scale analysis. Eschewing the easy cynicism and condescension, so evident in earlier studies, Robert Weisbrot depicts Divine as a man strongly impelled by a passion for racial equality and justice and the Peace Mission movement as a socially progressive religion. Based largely on Divine's letters and sermons, on *New Day*, the major journal of the Peace Mission, and on the author's interviews with movement adherents, *Father Divine and the Struggle for Racial Equality* supercedes the once-popular contemporary biographies of this evangelical preacher by Robert Allerton Parker, John Hoshor, and Sara Harris that focused on the comical and the sensationalistic and helped perpetuate the myth of Divine as a demagogue and the Peace Mission as an "escapist" cult.

With admirable brevity, Weisbrot's study explains how the crisis of the Great Depression transformed the emphasis of the black church from the spiritual realm to social reform and how Divine forged his Peace Mission into an effective vehicle in the struggle for racial justice. It shows Divine, battling both white bigots and black conservatives, defying restrictive housing covenants and actively promoting residential integration, even moving some of his flock from the slums of Harlem and Newark to an estate bordering Franklin Roosevelt's Hyde Park

mansion. It portrays Divine's flourishing network of economic cooperatives as an embodiment of his faith in ambition and the work ethic and his deprecation of government charity or welfare. Perhaps the book's most unique contribution is that it demonstrates that Divine fought for most of the civil rights causes of the 1930s. Moreover, Divine truly made his racially integrated Peace Mission into a concrete example of interracial harmony and equality.

Interspersed in Weisbrot's account are some brilliant observations on such diverse matters as charismatic leadership, black religion, and the nature of democratic reform. The author, for example, characterizes the black church as a mirror of evolving black hopes and values, adapting to changing communal needs, rather than an institution inherently either conservative or progressive. His comparisons of Divine and Marcus Garvey and the Peace Mission with the Nation of Islam are alone worth the cost of the book, as is Weisbrot's explication of how Divine's theology led him to insist that the struggle for racial justice was the highest religious calling.

This reviewer could quibble that much of the first half of the book adds little new knowledge. The paucity of documentary evidence forces Weisbrot, in the main, to restate earlier narratives of Father Divine's early life and the rise of his messianic ministry—although he surely does so in a more studious manner than previous biographers. I might add that occasionally, but rarely, Weisbrot overstates the importance of his subject in the quest for civil rights in the thirties. Father Divine had a role in the struggle, but he almost never conferred or corresponded with any of the major civil rights organizations, national leaders, or politicians sympathetic to the cause about matters of tactics or strategy, litigation or legislation. Rather, Divine's importance to the development of black militancy was primarily as a model of a clergyman who would not separate religion from the struggle for equality. That influence on race relations, as Weisbrot notes, would become increasingly visible and decisive in succeeding decades.

Father Divine and the Struggle for Racial Equality is a superb biography of a remarkable preacher. It adds a vital chapter to our collective history of black religion and struggle, the Great Depression, and social reform. Amen.

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HUMBERT S. NELLI. *From Immigrants to Ethnic: The Italian Americans*. New York: Oxford University Press. 1983. Pp. viii, 225. \$24.95.

One school of historians of Italian immigration to the U.S. distinguishes among those who emphasize

American influences, those who focus on Old World factors, and those who favor some blend of New and Old World elements in exploring their subject. Humbert S. Nelli's *Italians in Chicago* (1970) won him recognition as a leading representative of those who stress the American environment as the most significant element in the immigrant story. In *The Business of Crime* (1976) and in his latest book, *From Immigrants to Ethnic*, he has steadfastly maintained this "environmental" position. Be it choice of rural or urban settlement, family life, religious observance, economic adjustment, criminal activity, or the emergence of ethnicity, Nelli's telling of the story includes a prominent "made in America" stamp.

This latest and shortest of his books (less than 200 pages of text) extends in time from Christopher Columbus to Francis Ford Coppola. His greatest attention is, however, as it was in his earlier work, on the period from 1890 to 1940. Those familiar with his earlier work will find his previously articulated positions restated here, sometimes with new illustrations and at other times repeated virtually verbatim from the earlier books.

Nelli's first book appeared at the beginning of a very active period of historical study of Italian immigration. Some of the best of this recent scholarship has adopted an interactionist or an Old World perspective placing special emphasis on the social and cultural heritages of the immigrants. Significant contributions to the social history of rural Italy have followed as natural consequences of this concern for the Immigrants' Old World heritage. Nelli carefully avoids reference to this recent literature that could be considered hostile to his view of the immigrant experience. Readers of *From Immigrants to Ethnic* will find no references to Rudolph Vecoli's work on immigrant religion and radicalism, to Josef Barton's comparative study of social mobility among different peasant immigrant groups, to Dino Cinel's exploration of the effects of differing regional origins for Italians in San Francisco, to Virginia Yans-McLaughlin's study of Italian families, or to William DeMarco's argument for the importance of *campanilismo* in the North End of Boston. This is a fair sample of the recent work that finds no place in Nelli's footnotes or bibliographic note. As a result the book is, as Robert Cross notes on the dust jacket, neither "defensive" nor "aggressive."

This civility, pleasant though it is, comes at a price. Two unfortunate consequences follow from this strategy. First, it limits the book's usefulness to general readers by depriving them of the knowledge of serious alternative interpretations. Specialists will recognize the junctions between Nelli's narrative and other literature in the field, but this does not alleviate the damage caused by these omissions. By ignoring the existence of a substantial literature of alternative views Nelli has deprived himself of a

means to fashion his evidence and arguments to address most powerfully the issues in dispute. Rather than refining his case for his interpretation of the immigrant experience in light of other recent work, he tends simply to restate it. This is unfortunate. As often as not I find Nelli's positions have merit, but he has not reentered them into the debate in their most forceful form.

As an introduction to the Italian immigrant experience for general readers Nelli's book stands high among the available candidates. I regret that it is not the summary and synthesis that is needed after more than a decade of prolific, but specialized and often contentious, scholarship.

JOHN W. BRIGGS
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RICARDO ROMO. *East Los Angeles: History of a Barrio*. Austin: University of Texas Press. 1983. Pp. xii, 220. Cloth \$22.50, paper \$8.95.

East Los Angeles has been called "the capital of MexAmerica" because over one million Mexicans, Mexican-Americans, and Chicanos now live there, making it the leading community of Mexican descent in the United States. The bulk of *East Los Angeles: History of a Barrio* describes the establishment of this community from 1900 to 1930. One short chapter sketches out the barrio's development to the twentieth century; a brief afterword of nine pages carries the story from 1930 to the 1970s.

Ricardo Romo has written a study of urban history from the bottom up. The Mexican barrio grew principally to the east of the old Mexican plaza of Los Angeles. The new home of the immigrants was a source of personal strength and cultural identification as well as an acculturation way station to the host society. Migration from Mexico, along with employers' use of the barrio as a labor pool, gave rise to an ethnic enclave of over ninety thousand people by 1930, a year in which the total population of Los Angeles numbered more than one million.

The author does not go beyond the evidence of his work, properly calling for more case studies of immigrant communities to test commonly accepted generalizations. Most of the evidence and interpretations found here do, in fact, support what previous writers have argued about the Mexican in America. Economic and political dislocations in Mexico caused massive emigration from ancestral areas and forced an untold number of Mexicans, especially from the laboring classes, to flee to the safety of the United States. The barrio met the seasonal and year-round needs of employers who sought workers to fuel the burgeoning economy of California. Characterized by substandard housing,

inadequate sanitation facilities, and so on, the barrio provided affordable, if not decent, accommodations.

The book is at its best in dealing with relations between the immigrants and members of the wider Anglo-American society. The "Brown Scare" from 1913 to 1918 was a nativist campaign sparked by fears, often irrational, of Mexican radicalism and possible alliance with German agents in southern California. This chapter is necessary reading for those who want to understand the place of the Mexican in our society. Negative attitudes of many Americans also helped lock the Mexican immigrants into patterns of wide spatial mobility and narrow occupational choice. California was not the land of opportunity for all its inhabitants.

The internal dynamics of the immigrant community give evidence that life was not one long siesta until recent times. Despite Americanization pressure from Progressives and Protestant missionaries, barrio workers organized for self-protection and identification with their homeland. Mutual aid societies, patriotic festivals, and charity and self-help organizations did much to ensure group cohesiveness and slow down the assimilation process.

Romo has told well the story of Mexicans in Los Angeles and their great contributions to southern California's cultural and economic development in the early twentieth century. This case study of a heretofore neglected topic will hopefully stimulate scholars to scrutinize other urban barrios.

LAWRENCE A. CARDOSO
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ROBERT F. BYRNES. *Awakening American Education to the World: The Role of Archibald Cary Coolidge, 1866-1928*. Notre Dame: University of Notre Dame Press. 1982. Pp. xiii, 302. \$21.95.

"Archie" Coolidge's life at Harvard from 1893 to 1928 was that of a complete institutional man. He took over Edward Channing's History I and continued its successful undergraduate run until 1904. He was a popular and influential housemaster. He directed twenty-three doctoral theses, including those by Sidney Fay, Robert Kerner, Robert Lord, Laurence Packard, Dexter Perkins, and Lothrop Stoddard, and he helped guide the work of Frederick Artz, William Langer, Henry Leach, Richard Newhall, and almost three dozen more graduate students. He worked to expand the offerings of the department from three to fifteen fields of history. And, most of all, he constantly was collecting a great library for Harvard, housing it in the new Widener building. His devotion to buying and cataloguing, at his own expense, books in worldwide fields made a treasure house of Widener. On his way to Siberia in

1895 he purchased the entire Slavic holdings of a Leipzig bookstore; he captured a Munich professor's splendid collection on Bavaria and the Rhineland; he paid the salary and travel expenses of the agents who found and shipped back to Harvard sizable new additions in European, Russian, Latin American, and East Asian history. His motto, permitted him perhaps because of great personal wealth: "Buy first and find the money afterwards," paid off handsomely.

He was increasingly the public historian whose horizons were overseas. By 1909 he had crossed the Atlantic twenty-six times. He encouraged indecisive, well-heeled, and well-bred seniors to enter the Foreign Service where they formed a "Coolidge School" of diplomats and public servants—a self-assured, genteel, and efficient although inbred cadre guiding American affairs abroad. Coolidge himself became a member of the Inquiry and thereafter a member of the Commission to Negotiate Peace, forwarding from Vienna to Paris some 285 "sane and moderating" reports on conditions in Austria-Hungary. He was an early director of the Council on Foreign Relations and for six years the first editor of its prestigious *Foreign Affairs*, ably assisted at the council's New York office by Hamilton Fish Armstrong, with whom Coolidge corresponded daily. But, after Harvard, Coolidge's first love was the American Historical Association, whose meetings he attended indefatigably, and he became its principal reviewer of foreign language books on Russian and East European history from 1897 to 1910. Who else could have been chosen but Coolidge, the father of Russian and Slavic studies at the American university?

A critical modern historian may see this man for all Victorian seasons, this "first Brahmin professional historian," as one who, though an individualist, early defined professionalism even more comprehensively than did Herbert Baxter Adams or J. Franklin Jameson. And the critic of course will note Coolidge's ethnic prejudice and blindness to cultural and economic considerations in history while stressing international power politics. Robert F. Byrnes, himself in the Coolidge-Langer-Packard lineage, mentions these shortcomings in this detailed and faithful biography. If his book is generally uncritical and a bit repetitious, it nevertheless is important and welcome on the too-short shelf of American academic biographies. It tells us much more than the good study of Coolidge as librarian by William Bentinck-Smith (1976) and the sketch by Robert A. McCaughey (1979). This person, whom Harold Nicolson called "a humane and brilliant man," must be remembered.

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GEORGE H. NASH. *The Life of Herbert Hoover: The Engineer, 1874–1914*. New York: W. W. Norton or George J. McLeod, Toronto. 1983. Pp. xii, 768. \$25.00.

Writing in 1963, the Australian historian Geoffrey Blainey urged fuller study of Herbert Hoover's "forgotten years," a period, he noted, that had become overlaid with myth and misinformation. Since then biographies by Joan Hoff-Wilson and David Burner have helped to correct this situation, the latter in particular bringing much fresh information to bear on Hoover's mining and business career. But going beyond anything yet published is this first volume in a projected multivolume biography being written by George H. Nash with the backing of Hoover Presidential Library Association. Covering in copious detail the first forty years of Hoover's life, it seeks to document and understand not only his rise to prominence as a businessman and engineer but also the emerging social philosophy that would guide his subsequent public career. And these are matters, Nash insists, that are well worth understanding. For him, as for the British historian Denis Brogan, Hoover ranks with Theodore Roosevelt, Woodrow Wilson, and Franklin D. Roosevelt as one of the four most influential men in American political life between 1901 and 1945. Yet unlike them, he still lacks a full-scale multivolume biography.

To our knowledge of Hoover's first twenty-three years, Nash adds relatively little. But about the business career, which began in Australia in 1897 and ended with the outbreak of war in 1914, he has amassed more and better information than any previous scholar; and interspersed with the story of the businessman are rich chapters on Hoover's scholarly, philanthropic, civic, and professionalizing activities. One learns in detail about the precise role that Hoover played in Chinese mining development and the controversial Chinese Engineering and Mining Company, about his transformation of Bewick, Moreing into a new kind of highly successful mining firm, about the mineral discoveries and financial promotions that became the basis of his fortune, and about the numerous other business and nonbusiness projects into which he poured his abundant energies. One learns also about the frustrations and failures, about the difficulty Hoover had in acknowledging these, and about his tendency following such failures to minimize his own role, find others to blame, and credit himself with a foresight and detachment not borne out by the record. Although Nash does not say so, it was a pattern that would persist in his later public career.

In general, the Hoover emerging from Nash's work is closer to the one appearing in recent revisionist historiography than to those found in the

older Progressive history and its conservative rival. Nash's Hoover, like Burner's or Hoff-Wilson's, has his unattractive side. He was a man who encased himself in a protective reserve, loved to pull wires behind the scenes, quarreled repeatedly with associates and superiors, was often annoyingly insensitive, defensive, or self-righteous, and sometimes profited from the very business practices he had condemned as antisocial. Yet he was also, Nash shows, a man of truly remarkable intelligence, energy, organizational skill, and moral sensibility, who used his talents not only to make a fortune but to create a series of sound business enterprises, promote his profession, and perform a variety of "good works." His reputation in 1914 for progressiveness, economic expertise, and social responsibility was in large measure deserved. And behind the protective reserve, for those who could penetrate it, was a man capable of forming close ties with others, being remarkably generous and sentimental, and mixing seriousness with humor.

Like Burner's biography, Nash's study also sheds considerable light on the beginnings of Hoover's peculiar brand of reformism. He was never, so Nash concludes, a champion of laissez-faire or of orthodox free market economics. His vision instead was of a new capitalism ordered and kept progressive through professionalized organizations accountable to an informed public opinion. And in line with this vision, he became an early defender of the new managerial corporation, the modern trade and professional association, the science-based professional school, and a professionalized press. By 1909 he had also found a place in this vision for the businesslike labor union. But yet to come were the roles he would eventually prescribe for the professionalized community structure and the governmental agency functioning as institutional midwife and coordinator. During this period his experience with community groups and governmental action, particularly in Australia, had led him to see them in essentially negative terms.

Unlike Burner, though, Nash tends to downplay the Quaker influences on Hoover's thought and the impact of orphanhood on his psyche. Although he concedes that Quakerism may have helped to shape personal demeanor and outlook, he notes that Hoover early resisted its teachings and that from his college days on religion did not play a very significant role in his life. More important to his yearning for useful service, Nash believes, was the "Stanford spirit" of "practical idealism" so deeply imbued in Hoover during his impressionable undergraduate days. And more important psychologically than orphaned status itself, Nash suggests, was Hoover's relationship as an adolescent with his uncle, John Minthorn. It was in this often unhappy relationship that one may find the "genesis of his later pattern of

dealing with the world" (p. 572), especially his love of behind-the-scenes wire-pulling, his preference for indirection, and his enjoyment of quietly bending men to his purpose.

Nash's book is clearly a milestone in Hoover scholarship and seems likely to become the authority on Hoover's once "forgotten years." It can and undoubtedly will be faulted for its overinclination toward descriptive detail, which although often rich and fascinating becomes in some places excessive and tedious and in others repetitive. But much of this is probably unavoidable given the author's determination to be thorough and his need to examine closely the episodes and allegations featured in the Hoover smear literature of the 1930s. Throughout, the quality of the scholarship, the sifting and use of evidence, the imaginative reconstruction of relevant historical contexts, and the ability to communicate all deserve high marks. The author merits commendation for a fine piece of research and writing, and the Hoover Library Association praise for making its production possible.

ELLIS W. HAWLEY
University of Iowa

JAMES B. ATLESON. *Values and Assumptions in American Labor Law*. Amherst: University of Massachusetts Press. 1983. Pp. x, 240. Cloth \$25.00, paper \$12.00.

James B. Atleson sets himself the task of forcing the labor community to examine the received wisdom and hidden values that determine the working of this country's labor laws. His well-written, thoughtful book accomplishes this objective by taking nothing for granted and by analyzing the phrases, expressions, and doctrines used, without much thought, by the National Labor Relations Board and the courts. He asks us to think about what the 1935 Wagner Act was designed to accomplish and what it actually has wrought.

The Wagner Act was passed during a period of great industrial strife to lessen the number of strikes through strengthened collective bargaining rights for unions. But the strike is the only real power that labor has at the bargaining table. This is one of several major paradoxes that Atleson finds running through our labor laws. The act's other purpose—industrial democracy—has had so few gains that if the labor-management community were a banana republic, some congressional committee would now be at the point of cutting off foreign aid.

Atleson probes labor's weak role in the collective bargaining process and points to the limitations on the right to strike as the reason for its weakness. The labor board and the courts have outlawed the sitdown, the slowdown, and disdained the wildcat and the sympathy strike by, among other things,

allowing employers to permanently replace any worker who strikes to achieve an economic goal, that is, to get a good contract. Why? The author presents some of the underlying rationales of government labor policy, largely ignored by historians. The New Deal's assumption that worker actions should not pose a threat to the economy or its profit mechanism can be traced to the country's earliest employment relationships. Slaves and bound laborers, such as apprentices and indentured servants, made up the majority of the work force prior to the Revolution and set the basis for the New Deal's paternalistic view of laborers. The legal term for employment was, and often still is, a "master/servant" relationship. Loyalty and deference to the employer is still expected under our labor laws. For instance, the labor board upheld the firing of some television technicians who made what the board termed "vitriolic attacks" during a labor dispute by giving out handbills that criticized the television station for poor quality programming and second-class service. The Supreme Court commented, in upholding the board, "There is no more elemental cause for discharge . . . than disloyalty to his employer" (p. 85).

The 1947 Taft-Hartley amendments to the Wagner Act were passed at a time when employers feared that unions would be able to encroach on managerial prerogatives to become equals. Unions have never come close to justifying those fears. Atleson shows how labor has been barred from bargaining over some of the most basic issues that govern the working lives of most employees. Because, for example, management has the right to decide where it will invest its capital, it has followed that labor has no right to bargain over either plant closings or subcontracting. Management alone decides the basic direction of its enterprises, and labor can only watch as "their" company is sold out from under them and their jobs are moved across the country or across the world. The board does require that the employer bargain over the effects of the move, but not the move itself. This is one of the several major failings that Atleson finds in the collective bargaining system. He does an excellent job of exposing the roots of these failures. Anyone who is interested in the survival of a strong collective bargaining system will be wiser, but sadder, for reading this book. [These views are those of the reviewer and do not necessarily reflect those of the NLRB or its General Counsel.]

B. PATRICIA DYSON
National Labor Relations Board,
Washington, D.C.

JUDITH SEALANDER. *As Minority Becomes Majority: Federal Reaction to the Phenomenon of Women in the Work*

Force, 1920–1963. (Contributions in Women's Studies, number 40.) Westport, Conn.: Greenwood. 1983. Pp. xiii, 201. \$27.95.

This book is essentially a history of the Women's Bureau from its creation in 1920 to its eclipse in 1963. Based on the correspondence, files, and published and unpublished research of the Women's Bureau, and supplemented by secondary sources, it presents a complex and lively picture of this important institution. Its chapters lead one from the founding of the Women's Bureau in the early twenties, to its struggles in the Depression to defend women's right to work, to its active yet secondary role in the World War II mobilization of women workers, to finally its reorganization, shifting policy emphasis, and decline in the fifties and early sixties. Two intertwined themes emerge: the transformation of policy from protection to equal rights, and the position of the bureau as "outsider as insider."

The Women's Bureau, created in response to the pressure of women's groups such as the Women's Trade Union League and the Settlement House movement, embodied from the start Progressive reformers' commitment to protecting women as present and future mothers from harsh conditions in the labor force. This led the bureau to focus on exposing poor working conditions and to advocate protective labor legislation. It also explains the Women's Bureau's opposition to the Equal Rights Amendment that it thought would deny women a special status in the labor force, and push them into competition with men for which they were unequipped. This view underlay bureau policy until the fifties when it was replaced by a conception of women wage earners as independent individuals with the right to a free choice of jobs. This new stance led the bureau to give up policies that separated and protected women in favor of those that would remove restrictions on their employment. Equal pay and antidiscrimination measures replaced protective legislation.

An associated theme is the "outsider as insider": although part of the government, the Women's Bureau always remained an outsider to its workings. Under-funded, without any policy-making power, and excluded from the "old boys networks" of employers, unions, and government bureaucrats, the all-woman staff of the bureau used influence, hard work, and its constituency of middle-class women's organizations to further its cause. Although it had some successes, notably in disseminating information on low-paid women factory workers, the Women's Bureau's counsel was usually ignored by the men in power who, Judith Sealander shows, were uncomfortable with its female professional staff as well as its subject of women in the labor force. Ironically, the bureau feuded with a

potential ally, Frances Perkins, the first female head of the Department of Labor.

Sealand's focus on the Women's Bureau prevents her from presenting a coherent analysis of policy. For example, the reasons for the shift in policy from protection to equal rights remain elusive, since insufficient attention is given to the historical changes in women's relationship to the labor force, in the goals of women's organizations, and in general labor-force policies. Nevertheless, the book provides a wealth of heretofore inaccessible information that is certain to enrich our understanding of government policy making regarding women in the labor force.

JULIE MATTHAEI
Wellesley College

NORMAN DAIN. *Clifford W. Beers: Advocate for the Insane*. (Contemporary Community Health Series.) Pittsburgh: University of Pittsburgh Press. 1980. Pp. xxix, 392. \$19.95.

Clifford W. Beers: Advocate for the Insane chronicles the life and work of the founder of the mental hygiene movement. After graduating from Yale, Beers succumbed to a manic-depressive mental disorder and experienced first-hand the patient neglect, institutionalized abuse, and custodial care typical of both state and private mental institutions in the early twentieth century. While still hospitalized, he began work on *The Mind That Found Itself*, a powerful exposé of asylum abuses, which he published in 1906, three years after his release. As Norman Dain notes, Beers was not the first former mental patient to crusade for asylum reform. Over thirty years before, Elizabeth Packard had launched a similar campaign to protect patient rights. But Beers accomplished far more than Packard and previous reformers, Dain argues, due to his superior social connections and ability to harmonize his cause with larger social concerns. Using *The Mind That Found Itself* to publicize his efforts, Beers won the moral and financial backing of leading psychiatrists, reformers, and philanthropists, who recognized the potential of a popular mental hygiene movement to enhance the status of scientific psychiatry and to improve social efficiency and stability.

Dain concentrates primarily on Beer's work for the National Committee for Mental Hygiene (NCMH) that he founded in 1909 to better conditions in American mental hospitals and to provide up-to-date information about mental illness and mental hygiene. With funding from the Rockefeller Foundation and private philanthropists, the NCMH developed a standard disease nomenclature and hospital survey form for the U.S. Public Health Service, set up demonstration child guidance and

mental hygiene clinics, and published a journal devoted to mental hygiene issues. Yet the NCMH never became the broad-based, activist organization that Beers had originally envisioned. Beers's difficult personality led to a long series of clashes with psychiatrists affiliated with the NCMH, which weakened its effectiveness. Dependence on funding from conservative donors further diminished the organization's capacity to develop innovative programs. When his own failing mental health forced Beers to return to institutional care in 1939, he was profoundly disappointed in the mental hygiene movement's accomplishments.

Still, within twenty years of his death in 1945, the community mental health program brought to fruition many of Beers's most cherished aspirations: substitution of community-based clinic care for institutionalization; greater regard for patient rights; and more open discussion of mental health issues. What a voluntary, privately funded organization could not accomplish in the conservative interwar period, a federally financed and directed program achieved in the activist 1960s.

Dain's book should be of interest not only to historians of psychiatry but also to those interested in twentieth-century philanthropy and reform. Through Beers's autobiography, Dain addresses the conflict between lay and medical reformers, the role of corporate foundations in shaping the provision of social services, and the larger fate of Progressive reform. He tends to dwell over much on the details of fund-raising campaigns and internal squabbles at the NCMH; the larger social and political context of Beers's life is slighted as a result. Still, Dain has written a solid, balanced biography of a fascinating reformer that will remain the standard work on Clifford Beers for decades to come.

NANCY J. TOMES
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Stony Brook

CAROLYN E. SACHS. *The Invisible Farmers: Women in Agricultural Production*. Totowa, N.J.: Rowman and Allanheld. 1983. Pp. xiv, 153. \$23.95.

It would be easy to overlook the significance of this raggedly revised dissertation. The social science method of incorporating a literature review into the text may be additionally jarring to historians. But if one can get by these obstacles, both useful information and insightful analysis is to be found.

Carolyn E. Sachs, a rural sociologist, tackles the huge task of examining the roles of women in agricultural production in the United States. She offers a historical summary that includes the Northeast, South, and West. The theme that emerges is the continuation of the patriarchal system despite

technological and other changes. She argues that "males have encouraged the sexual division of labor on the farm," partly to maintain their own power (p. 14).

Sachs demonstrates that the nineteenth-century ideology of domesticity further encouraged the division of labor by maintaining that women "should" be primarily involved in family and household matters. In industry, the domestic philosophy justified the use of women as a reserve labor force to be called into the factories as needed. In agriculture, it helped displace women from agricultural production. This situation could be clearly seen in the growth of "agricultural science" for men and "domestic science" for women. Today the agricultural sciences are characterized by job segregation that "pushes women scientists into domestic concerns, while men study agricultural production" (p. 60).

To explore the contemporary situation more thoroughly, Sachs interviewed twenty-one women involved in farming in Kentucky, Ohio, and Indiana. These included widows, single women, women married to nonfarmers, and women married to farmers. Sachs learned that women in modern agriculture are more involved in record keeping and information gathering than they were in the past, but that decision making still remains largely in the hands of men. Many of the respondents offered reasons for their failure to become "full-fledged" farmers. These included extension courses directed toward men, the refusal of banks and lending establishments to extend credit to women for land purchases, nonadjustable tractor seats and pedals that do not "fit" women, the continuation of domestic workloads, and exclusion from "jawing" sessions in which male farmers exchanged essential information.

There are many possible points of issue. The sample is small and concentrated in the Midwest, and the book itself attempts to cover too much material in too few pages. But it is more important to give attention to its conclusion. The author convincingly argues that American policy makers are transferring the male-dominated agricultural system to developing countries around the world through farming programs designed for men and home economics programs for women. Cutting women off from agricultural resources and the decision-making process does not seem to harmonize well with stated American values. Do we really intend to teach what we say we do not believe in?

GLEND A RILEY
University of Northern Iowa

AVI SHLAIM. *The United States and the Berlin Blockade, 1948-1949: A Study in Crisis Decision-Making*. Berke-

ley and Los Angeles: University of California Press. 1983. Pp. xiii, 463. \$38.00.

This excellent book, part of a series of works of the International Crisis Behavior Project directed by Michael Brecher of McGill University, seeks to test theories of decision makers' behavior under stress. As such, it is, from historian's point of view, overloaded with theory, models, and the scrutinizing thereof.

The bulk of the book, however, is a detailed, meticulously researched account of the origins and events of the Berlin blockade, airlift, and negotiations of 1948-1949. It is based on wide research in British and American archives, oral histories, unpublished diaries, memoirs, published documents, and all relevant secondary works.

Although the tale is in general a familiar one, Avi Shlaim refines it and reminds us of elements either unknown or too easily forgotten. For instance, General Lucius Clay made the fundamental decision to use an airlift as an expedient to avoid a rapid withdrawal from an isolated West Berlin. This gave the U.S. an option beyond those of ignominious retreat or use of force, as in a road convoy. President Truman's swift approval of that decision, made with almost no reflection, started the course of action that ultimately defeated the Soviet blockade.

Shlaim reminds us that the Joint Chiefs of Staff, perceiving limited American military strength, were more cautious than many civilians and fitted in with Truman's wish to avoid what we would later call "escalation." Faced with Soviet intransigence, Secretary of State George C. Marshall turned to the United Nations—although this approach was thwarted by a Russian veto. Finally, American leaders grasped the fact that the U.S. had little leverage with the USSR in other respects, such as stopping exports of machinery or closing the Panama Canal.

Shlaim is expert at revealing leaders' attitudes and interactions in making decisions. Truman, Marshall, Acheson, Lovett, Clay, Forrestal, and others emerge as personalities to a degree rarely achieved. Shlaim's accurate portrait of Truman includes the qualities of high tolerance for stress, swift assumption of personal responsibility, a nonbureaucratic approach in time of need, avoidance of delay or overconsultation, but a demand for accurate and adequate information. He pays due attention to Truman's reelection problems and his skillful use of bipartisanism to insulate the Berlin crisis from domestic politics.

The author is equally explicit on U.S. and Russian signals to each other. While he sees the U.S. as rather "hard line," he also notes Truman's caution regarding atomic bombs, keeping them in civilian hands when Forrestal was arguing for military control. One can also see in Shlaim's portraits signs of

Forrestal's impending mental collapse. Incidental, but useful, is a study of Stalin and Molotov playing the old game of "good cop" and "bad cop" to manipulate negotiations with the U.S.

Oddly, for a book from such a distinguished press, there are several spelling errors and "typos," and a few errors in usage. Worse, the font of type used evidently did not include umlauts for German names.

More serious, Shlaim misses some significant points. He omits Truman's problem with the right-wing "Dixiecrats," and in his discussion of the reasons for consultation fails to see the most important of all: finding the best option. There are some distortions in his account of the "postcrisis" period; he contends that the "crisis" ended with the American decision to use the airlift in order to remain in Berlin. Actually, the worst of the crisis ended when the USSR did not interfere with or block the airlift, thus eliminating any real threat of war. Finally, Shlaim errs in calling Truman a "lame duck" president; Truman was re-elected in early November and the blockade and airlift continued for six months afterward.

Shlaim, however, proves his point that political scientists' models need to be re-examined. He finds that some decision makers, at least, do not behave according to theory, that stress may not impair "cognitive performance." He concludes that in this case Marshall avoided using stereotypes to explain Soviet behavior and insisted on leaving the USSR some options. He finds that the American leaders sought relevant, not random, information; that they sought a variety of options and carefully evaluated those they found; that there was no "groupthink"; that there was no shift to a riskier course of action; that, under Truman's perhaps too quick but decisive leadership, there was no surrender of long-range goals (for example, the new West German republic) for the sake of a quick settlement.

ROBERT W. SELLEN
Georgia State University

BURTON I. KAUFMAN. *Trade and Aid: Eisenhower's Foreign Economic Policy, 1953-1961*. (Johns Hopkins University Studies in Historical and Political Science, number 1.) Baltimore: Johns Hopkins University Press. 1982. Pp. xiv, 279. \$25.00.

It is always a pleasure to review a book written by a true craftsman. As was the case with his previous works, this meticulously researched study of Eisenhower's foreign economic policy by Burton I. Kaufman is well written, clearly developed, and judicious in its conclusions. It will undoubtedly become a standard reference on the subject and should serve

as a model for similar studies of other periods in American history.

Kaufman's major thesis is inferred in his title. Dwight Eisenhower came into office in 1953 committed to "eliminating foreign aid and relying instead on liberalized world trade and the encouragement of private foreign investment to assure world economic growth and prosperity. Except for . . . military assistance to stop the spread of communism, Eisenhower's program focused on Europe rather than the Third World" (p. 7). Beginning as early as 1954, however, and continuing until the end of his administration, the problems of the Third World—"the rising tide of nationalism, the inadequacy of trade and private investment as a solution to its economic problems, and Soviet economic efforts among underdeveloped countries—caused Eisenhower to modify his thinking" (p. 7). Increasingly, the emphasis shifted to trade and aid and focused on the Third World as well as on Europe.

Kaufman is quite clear that the central motive behind this shift was a long-term determination to "wean Third World countries away from international communism and towards the West" following the French defeat in Indochina and the Soviet launching of an economic offensive in the Third World in the mid-1950s (p. 207).

He is equally clear that the assumptions behind this policy were—in retrospect—naïve. A sustained program of economic assistance "would create a preponderance of stable, effective and democratic societies—the last hope for a favorable settlement to the Cold War" (p. 97). "Such basic issues as the harsh circumstances of primary producing countries in declining commodity markets or the economic consequences of their social and political systems continued to go largely unnoticed or unheeded" (p. 115). The issues of the North-South dialogue of the 1970s were thus present but largely ignored in the 1950s.

One is also struck by the degree to which—despite the differences in rhetoric—there was a high degree of continuity between the policies actually carried out by Truman, Eisenhower, and Kennedy administrations. In part, this was a product of constraints placed on foreign economic policy by the role of Congress in appropriations. But one is also reminded of the axiom that "the national interests of the United States do not change abruptly every four years."

Eisenhower himself fares well in Kaufman's study, consistent with more recent interpretations. Kaufman sees him as intelligent, active, and—despite his low profile—definitely in charge in the administration. Kaufman also emphasizes the strength of Eisenhower's personal anticommunist convictions and the role this played in the formula-

tion of policy—especially in the reorientation of the mutual security program away from military aid and toward economic assistance, and the shift in the geographic direction of American foreign aid toward the Third World.

In short, this book is a solid contribution that will be valuable to historians and political scientists for a long time.

IRVINE H. ANDERSON
University of Cincinnati

GEORGE R. METCALF. *From Little Rock to Boston: The History of School Desegregation*. (Contributions to the Study of Education, number 8.) Westport, Conn.: Greenwood. 1983. Pp. x, 292. \$35.00.

George R. Metcalf, a journalist and civil rights activist, has written a profoundly disturbing book. Despite the sweep of its title, it is an examination of the events resulting from attempted enforcement of Title VI of the 1964 Civil Rights Act, the section empowering the Department of Health, Education, and Welfare to cut off federal funds from school districts that refused to desegregate. The work thus evaluates the school desegregation efforts of the Johnson, Nixon, Ford, and Carter presidencies with particular emphasis on the Nixon years.

The thesis of the work is that Nixon, once an advocate of civil rights, found it politically advantageous to change his position on school desegregation because of the demands of Republican strategy in the South, the challenge of George Wallace, shifting public opinion on school busing, and the emerging crisis over de facto segregation in northern urban schools. Consequently, Nixon pressured and eventually overcame the prointegration majority of the U.S. Supreme Court, crippled and confused HEW's enforcement of Title VI, caused the Justice Department to intervene on the side of delay in desegregation cases, and urged Congress to pass an antibusing amendment. The result of Nixon's skillful manipulation of public opinion, according to the author, was increasing resistance to school integration in both the North and the South that culminated in the violence accompanying the integration of the Boston public schools.

The author, however, does not propound a simplistic Nixon-devil thesis. He offers sharp criticism of community leaders, school officials, and local politicians for their failure to publicly express strong support for integrated schools. His description of the rapid turnabout by many liberal congressmen once busing became unpopular is especially telling.

The strong points of the work include clear, analytical discussions of such critical Supreme Court decisions as *Alexander v. Holmes* (1969) and *Milliken v. Bradley* (1974). The author's assertion that the

Court's rejection of cross-district school busing in the latter case was a critical turning point is undeniable. The restraint-oriented Burger Court's decision was not surprising, though, given its composition, the lack of controlling precedents, and overwhelming opposition to cross-district school busing.

The work has three significant flaws. First, it passes over with a few brief generalizations the reasons for the relative inaction of the Eisenhower and Kennedy administrations on school integration. Metcalf's assertion that Eisenhower never "embraced" (p. 3) school integration exemplifies this point. This view overlooks the complex forces converging on Eisenhower that made him very cautious about integration. Second, and most critical from a methodological point of view, is the author's heavy reliance on newspaper sources. There are critical places in this work, as in his assessment of Nixon's behavior, where there is a need to get beyond the words and actions that are a matter of public record. Third, although Metcalf describes the politicization of the school desegregation issue, he underestimates its controlling role. Nowhere is this role more evident than in the exploitation of antibusing and neighborhood school arguments.

In summation, this work is the most comprehensive account of the school desegregation controversy from the mid-sixties to the present. It is a telling reminder of the vast chasm between legal rights and actualities in our society where black children are concerned. Whether intentional or not, Metcalf proves that too much of the American dream is subject to the limiting effects of political expediency, an aspect of the political process that we seem unable to escape.

JAMES C. DURAM
Wichita State University

ELLEN CONDLIFFE LAGEMANN. *Private Power for the Public Good: A History of the Carnegie Foundation for the Advancement of Teaching*. Middletown, Conn.: Wesleyan University Press; distributed by Harper and Row, Scranton, Penn. Pp. xix, 246. \$17.95.

The announced purpose of this book is not only to relate the accomplishments of the Carnegie Foundation in the areas of pensions, standardized testing, and medical education, among others, but also to describe the foundation's relations "to the larger social issues of the twentieth century" (p. xii). Following brief biographical sketches of Andrew Carnegie and Henry S. Pritchett in Part 1, Ellen Condliffe Lagemann discusses the reasons that led Carnegie to provide funds for the establishment of a foundation in 1905 and for Pritchett to become its first chief executive officer (1905–30). The author then examines the launching of the Carnegie pen-

sion plan for college teachers, the ostensible reason for the setting up of the foundation, which eventually resulted in the creation of the Teachers Insurance and Annuity Association (TIAA) in 1917–18. She also shows how Pritchett and the educators on the foundation's board of trustees were determined to and did, use the pension plan to reform the U.S. higher educational system through the establishment of educational standards for the admission of colleges and universities to the plan.

Part 2 of *Private Power for the Public Good* presents an analysis of a few of the numerous surveys undertaken by the foundation from 1905 to about 1930—medical, legal, engineering, and teacher education—but omits others such as dental education and college athletics. Although Lagemann does not dispute the beneficial effects on segments of the higher educational system that resulted from the various reports and studies, she downplays the traditional significance accorded them. For example, the Flexner report on *Medical Education in the United States and Canada* (1910) is pictured as primarily supporting and accelerating a movement for medical educational reform that was already well under way and that was led by certain leaders in the American Medical Association (AMA). Lagemann gives them almost equal credit with Pritchett and Abraham Flexner. This view contrasts with that expressed in an earlier history of the foundation that simply makes reference to Flexner's consulting AMA leaders and then states that it was Pritchett and Flexner who had to withstand a storm of vituperation on publication of the Flexner report. This same history also refers to Flexner as "the Father of Modern Medical Education" and says that "he has probably exercised more influence upon the course of medical education all over the world than any other one man" (Howard J. Savage, *Fruit of An Impulse: Forty-Five Years of the Carnegie Foundation, 1905-1950* [1953], pp. 105–08).

There follows in Part 2 a discussion and explanation of the development of testing agencies between 1900 and the 1940s. The important roles of the Carnegie Foundation and William Learned, Ben D. Wood, Edward L. Thorndike, and Carl C. Brigham are woven into this account.

Dismissing Carnegie Foundation activities from the 1930s to 1967 as "years of relative quiescence and decline" (p. 123), the last section of Part 2 is entitled "Renewing the Foundation." This section is primarily a highly laudatory account of the activities of the Carnegie Commission on Higher Education (1967–73) and its successor, the Carnegie Council on Policy Studies in Higher Education. Set up largely at the instigation of Alan Pifer, president of the Carnegie Corporation of New York from 1966 to 1982, these bodies conducted "studies that were in style, consistent accuracy and high caliber, sub-

stance, and even outcome not dissimilar from the more noteworthy of the ones the foundation had supported before" (p. 152). Lagemann's conclusion is that through the work of the commission and the council, the Carnegie Foundation for the Advancement of Teaching emerged as a reorganized foundation in 1979 and had reestablished "itself as an agency of rational standards in American education and American society" (p. 153).

Part 3, entitled "The Great Society," discusses criticism of Pritchett and the foundation by his contemporaries and presents Lagemann's own evaluation of Pritchett and the early work of the foundation. She is critical of the political-social views held by Pritchett in the 1920s and 1930s, lumps them with those held by President Herbert Hoover, and dismisses them as "narrow and inflexible." In this connection she observes that Pritchett could not see a "need for deliberate and even coercive state action to balance liberty with equality" (p. 177). Lagemann concludes that Pritchett's successors as head of the Carnegie Foundation recognized the shortcomings of this approach and took steps to overcome some of them. The volume ends with a bibliographic note, notes, and an index.

Although well-written and provocative, the book is primarily an interpretation by the author of the role that a foundation (private power) should play for the public good rather than a complete chronicling of the activities of the Carnegie Foundation from its establishment to the present.

JOSEPH C. KIGER

University of Mississippi

GARY O. LARSON. *The Reluctant Patron: The United States Government and the Arts, 1943–1965*. Philadelphia: University of Pennsylvania Press. 1983. Pp. xvi, 314. Cloth \$30.00, paper \$12.95.

A chart showing federal support for the arts during the twentieth century would feature two peaks. The first would reflect the arts programs of the New Deal. The second would rise dramatically after September 1965 when President Lyndon B. Johnson signed the National Foundation on the Arts and Humanities Act. Graphically, the impression would be left that from 1943 to 1965 "nothing happened." Gary O. Larson demonstrates that such an impression would distort reality.

The arts programs of the New Deal, Larson contends, so violated the tradition of cultural *laissez faire* that they could not be sustained. When Congress and World War II brought them to an end, there began a series of cautious efforts to fashion a new relationship between the arts community and the federal government. Eventual success, in the form of the National Endowment for the Arts and

the National Endowment for the Humanities, resulted from the slow evolution of a plan, a rationale, and a movement. Much of the process was long-winded and the course tortuous. Through it all, opponents of federal patronage held that it was foolish to spend money on nonessentials in the face of a growing national debt. They believed that arts patronage was the responsibility of the private sector and that cultural affairs were the responsibility of state governments. Advocates of federal assistance shared the view that government should disseminate American culture abroad (usually expressed as "keeping up with the Russians"), elevate the electorate, improve the quality of leisure, and serve as catalyst for greater private involvement.

Presidents Harry S. Truman and Dwight D. Eisenhower declined to participate extensively in the debate among congressmen and arts advocates about the proper relationship between the arts and government. Still, almost every session of Congress considered some new or revived scheme. As a result, a long legislative history and the existence of an experienced block of congressmen made it impossible to ignore arts subsidies when world events and national attitudes changed.

Larson argues convincingly that "factors inherent in the political process—timing, practicality, association, leadership" affected the outcome of initiatives for an arts subsidy as much as did lofty ideals and real needs among the arts community. By the early 1960s Americans needed reassurance—of the sort that could be gained by showcasing artistic excellence—that the nation had not lost its way. Too, President John F. Kennedy created an atmosphere in which the arts seemed to matter. The desire to leave memorials to Kennedy after his assassination, the sudden activism of humanists and the academic community, and Lyndon Johnson's vision of a Great Society presaged the National Foundation on the Arts and Humanities Act.

The Reluctant Patron is impressively researched, and despite the complexity of events and the number of protagonists, reads well. Larson's perspective is that of an advocate as well as an analyst. Even so, he deals honestly with critics of arts subsidization. The book very competently fills a gap in American social and cultural history.

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MEL HORWITCH. *Clipped Wings: The American SST Conflict*. Cambridge: MIT Press, 1982. Pp. x, 473. \$25.00.

Mel Horwitch has provided an important and revealing study of a crucial episode in the history of

modern American technology. His examination of the controversy surrounding the decision not to build an American supersonic transport has broad implications for all students of technological planning.

In the late 1950s and early 1960s it was generally assumed that the United States would produce a passenger airliner operating at supersonic speeds. The technology was in place. Industry was capable of producing the craft. Government officials seemed prepared to assist in underwriting the venture. Moreover, Soviet and European airframe manufacturers were moving forward with their own SST programs. Surely the American manufacturers who had so long dominated the market would not be left behind.

The time shaved from transoceanic travel would, of course, be purchased at a cost. Noise levels at takeoff and landing would be significantly higher than in the case of conventional aircraft. The sonic boom would present a problem, and there were serious concerns regarding the impact of scheduled, high-altitude flight operations on the ozone layer. Some of these problems would be solved. Those that were not would be accepted as the price of technological progress—or so it was assumed.

But the public decided otherwise. A broad coalition of citizens groups focused attention on the central environmental issues and lobbied effectively against government officials and industry figures supporting construction of the SST. At the same time cost analysts were pointing to the harsh economic realities faced by air carriers operating on SST. Soaring fuel costs, noise restrictions, and other factors would limit the SST to use in very long-range service. This fact, coupled with the speed of travel, would, in turn, drastically limit the market for SSTs to perhaps twenty-five to thirty aircraft in international service.

By 1971 this combination of environmental and economic concerns had brought the American SST program to an end. Subsequent experience with the Concorde has amply demonstrated the ultimate wisdom of the American decision.

Clipped Wings focuses on the actions and attitudes of important participants in the decision-making process, from Presidents Kennedy, Johnson, and Nixon to Robert McNamara, FAA Administrator Najeeb Hallaby, and environmental activist William Shurcliff. The book is extraordinarily well documented. In addition to the wealth of material culled from government files, including several documents obtained through the Freedom of Information Act, the author has interviewed many of the leading figures in the controversy. In view of the extraordinarily full annotations, the absence of a bibliography is scarcely noteworthy.

Horwitch finds it difficult to suppress a tendency

to overwhelm the reader with detail. As a result, the volume is not easy reading. As one of the few good case studies of the decision-making process in modern technological planning, however, it is well worth the effort.

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DESMOND BALL. *Politics and Force Levels: The Strategic Missile Program of the Kennedy Administration*. Berkeley and Los Angeles: University of California Press. 1980. Pp. xxvi, 322. \$27.50.

From a political scientist's perspective, history repeats itself. This is not because mankind is incapable of learning (especially from mistakes) but because institutions and the traditions of strategy and planning associated with them are more stable and compelling than the careers of men and women who make policy. So, the superb history that Desmond Ball has written of the evolution of John F. Kennedy's strategic missile program should be of interest both to scholars of that period as well as to those who seek to understand the deeper causes of the trouble the Reagan administration is having deciding how to compose and deploy a new generation of strategic forces.

Ball's account of the Kennedy era is based on archival and interview research, including off-the-record discussions with more than fifty former officials. It seeks to explain the strategic program adopted by reference to the decisions taken in the last three years of the Eisenhower administration, the impact of McNamara's whiz-kids and their approach to strategic and budgetary planning, and the dynamics of the controversy surrounding the "discovery" of a U.S.-USSR missile gap. Each of these facets is thoroughly explored and, more importantly, well balanced. The conclusions emerge forcefully from the analysis.

Read today, *Politics and Force Levels* is a potent reminder of the importance of the political dynamics that shape strategic decisions. Ball's conclusion is that "the outcomes of the decision-making process were not wholly or solely the results of objective and systematic analyses," but reflected "more than anything else, the relative powers of the participating groups" (p. 269). It is also, I suspect, precisely what will be said twenty years from now when Ball or his colleagues at the International Institute for Strategic Studies explain what happened and why in the early 1980s when Ronald Reagan tried to close the so-called "window of vulnerability" that led, in part, to his election in the first place.

ALLAN E. GOODMAN
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CLAYTON R. KOPPEL. *JPL and the American Space Program: A History of the Jet Propulsion Laboratory*. New Haven: Yale University Press. 1982. xiii, 299. \$19.95.

Most Americans first learned of the Jet Propulsion Laboratory when its astounding Vikings and Voyagers scooped soil on the "red planet" to look for life, toured Jupiter and Saturn, even exited the solar system—all at the command of technicians up to 1.5 billion miles away. By then, however, the JPL was already in its fourth decade, and its history adumbrated that of rocketry and spaceflight in the United States.

Clayton R. Koppes argues convincingly that the Guggenheim Aeronautical Laboratory at Caltech, not the lonely work of Robert Goddard or the Von Braun team, was the real seedbed of American rocketry. During World War II, the GALCIT became JPL, moved from basic research to missiles as part of the postwar "scientific-military-industrial complex," and helped the Huntsville Germans launch Explorer 1 in 1958. But Sputnik had been first, and the space race to follow steered JPL on its course toward the planets. NASA replaced the army as the source of funds and missions, but administrative conflicts and the repeated failure of its Ranger and Surveyor lunar probes eroded JPL's proud independence. The "space age management" techniques of NASA chief James Webb then released the undoubted technical virtuosity residing at JPL until the lab emerged as the most prestigious fraternity of astronautical craftsmen. In the 1970s space science went begging, and by the 1980s the unique corpus of talent at JPL could survive only by turning again to classified research.

So far so good. But, the author tells us, this is not to be a strict institutional history, nor strictly a history of science and technology. Indeed, the book falls between these stools. The Corporal rocket is covered in depth, but the technical history of the planetary craft—JPL's pride—is glossed over. Similarly, he dwells on the problems of a university-affiliated lab doing military work, but says little of internal management (William Pickering's twenty-four years at the helm are only caricatured, not analyzed), or the problems of institutionalizing creativity. Still, he says his purpose lies elsewhere, so it is there that the book must stand or fall.

"This book is, then, a history of a national laboratory in relation to certain military and scientific policies in the national security state." The latter term is borrowed from Daniel Yergin, and the theme is nothing less than the "very real, and unresolved, tension . . . between these perceived national security interests and the maintenance of the country's democratic form of government" (p. xi). Koppes thus places the book within a revi-

sionist historical frame that is neither subsequently supported nor necessary to the argument as it pertains to JPL. It invites us to expect that the Caltech affiliate was coopted by the Pentagon and its academic values obviated. Thus we are surprised to learn that the leading lights of JPL, with few exception, welcomed secret military work. Why? The implications are that it was greed (JPL management fees amounted to 12 percent of Caltech's budget in 1963) or bellicosity. Clark Millikan is branded with the "ideological underpinnings of a conservative cold warrior" (p. 27), and Lee DuBridge ridiculed for adopting "the familiar Cold War defense" that weapons programs were "not the fault of Caltech or the military services but the fault of one Joseph Stalin" (p. 48).

It comes as another surprise, therefore, when we learn later that Pickering and DuBridge agreed in 1954 to move JPL out of major weapons work. The proffered explanation, that spaceflight was the coming thing, is based on a quotation dated 1957 (p. 82). Similarly, the Apollo moon program is interpreted as a nonlethal way of showing American "readiness to use its nuclear arsenal" and a prop for the aerospace industry, with a military payoff including "killer" satellites (p. 115). Here Koppes veers away from John Logsdon's sturdy interpretation of Apollo, but offers no evidence except Webb's budgetary boilerplate about the military importance of space and a memo written two years after the fact.

Editorial gratuities also detract. "Missiles and Mexico," we are told, "made White Sands perhaps the ultimate in machismo for JPL males" (p. 47). "Upper-middle-class residents of Pasadena objected to the noise and unsightliness of the Lab" (p. 48). James Van Allen's metaphor—"sending . . . scouting parties to several planets before we send out the wagon train with all of our women and children"—is deemed "sexist" (p. 188). And JPL, notorious for its prickly relations with patrons, is berated for overconfidence, condescension, swagger, élan as "offensive" as that of the marines, pride (twice), and arrogance (thrice) all in the space of thirteen lines.

Koppes is right to treat JPL as a case study in the governmental take-over of private research. But the author's uncritical revisionism skirts the hard questions of why the U.S. felt obliged to create a "national security state," why universities like Caltech welcomed their own "co-optation," what the alternatives were given the Soviet challenge, and whether spaceflight could have matured as anything other than a stepchild of military missilery? JPL and Caltech documents alone could not answer such questions in any case. Nevertheless, those files yield, and this book provides, much new information on the history of the spacecraft designers at JPL, who

felt they were, and according to Webb were indeed, the best in the world (p. 248).

WALTER A. MCDUGALL
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J. FRED MACDONALD. *Blacks and White TV: Afro-Americans in Television since 1948*. Chicago: Nelson-Hall. 1983. Pp. xvi, 288. Cloth \$23.95, paper \$11.95.

Americans have lived with television for over thirty years, yet only recently have scholars begun to plumb the nature and influence of the pervasive medium. Historians, particularly, have neglected such study, leaving the field to sociologists, journalists, and psychologists. No historical synthesis regarding television's evolving relationships with American society and culture exists to guide students through the thicket of contradictory assumptions and assertions coming from so many sources. Indeed, no agreement exists on what television really is, with one side claiming it is nothing more than a marketing medium subservient to advertisers and the other finding in it a source of "free" entertainment and information responsive to viewers' needs and preferences.

Amid this neglect and confusion, J. Fred MacDonald brings some order to one important aspect of television—the place of blacks in the predominantly white industry. Television has been especially significant in shaping the black world. Blacks watch proportionately more television than whites, for example. During its formative period, the medium enlarged the arena for civil rights activity, and, thereafter, it offered the promise of weakening racial prejudice and despair by presenting sympathetic treatments of black culture and positive black role models. MacDonald's book, the first substantial work on the subject, represents not only a scholarly enterprise but also an event of some immediate importance.

MacDonald charts the uneasy relationship between blacks and the television industry from the early days of the medium through the 1970s. The association, he concludes, has been ambivalent, at best. The high visibility of blacks on early television hinted at a day of bias-free programming, even though many black stereotypes from stage and film persisted, as in the *Amos 'n' Andy* shows. During the 1960s, however, blacks made television the "chosen instrument" for social change and scored numerous successes in bringing serious black talent to dramatic roles, black issues to newscasts, and black sensibilities to programming generally. The "30 billion dollar Negro" market combined with a liberal consciousness, partly galvanized by television's coverage of civil rights, changed the color and face of television

for a decade. In the 1970s the gains receded. Television abandoned social "relevancy" for amusement, and the old stereotypes of blacks as buffoons or sharpsters reappeared on the screen. Norman Lear and Flip Wilson come in for some harsh words from MacDonald on the latter point. For MacDonald, television failed in its responsibility to undo America's cultural legacy of bigotry by projecting "undistorted, honest information" about blacks. Television entered the 1980s unsure of itself and its obligations to society.

MacDonald does well in surveying the shifting contours of black images in television, and he is especially insightful in his examination of television journalism. Problems arise, however, with his facile reduction of the 1970s to an age of Nixon-like conservatism and his belief that the federal government alone must define civil rights. Information is power, but MacDonald forgets that in America information is also profitable. MacDonald is unwilling to concede much justice to the television industry's profit motives, rather expecting the industry to respond to social issues alone. He does not always realize that other groups besides blacks also decry the abuses of television and demand consideration for their peculiar interests when it is not always apparent what the interests are or who speaks for any group. Nor does MacDonald grapple with the oddity that however much television survives as a wasteland and offers unflattering, flat poses of blacks or other groups, the medium continues to increase its audience. Why do people watch television if the picture is not at all pretty anyway?

These criticisms aside, MacDonald has written a significant book that will end much silliness about the meaning of television and will invite further studies of television's place in American culture. More importantly by showing the failure of television's promise to blacks, to America really, he reminds us that mass media is more than mere entertainment.

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CANADA

THEODORE J. KARAMANSKI. *Fur Trade and Exploration: Opening the Far Northwest, 1821-1852*. Norman: University of Oklahoma Press. 1983. Pp. xxii, 330. \$22.95.

Much has been written about the discovery and exploration of North America, and the fur trade in particular has received attention. Theodore J. Kara-

manski correctly states that the Far Northwest—embracing the interior of northern British Columbia, the western Northwest Territories, the Yukon, and eastern Alaska—has been relatively neglected. The author fills this void in the historiography of the fur trade by telling the stories of the young Scots and Englishmen in the service of the Hudson's Bay Company and the North West Company who explored this vast area and traded for precious furs.

In 1821 the bitter rivalry between the companies ended when they combined forces. For over three decades, the North West Company had competed with the Hudson's Bay Company, but in the end its three thousand miles of canoe trails leading back to company headquarters in Montreal proved too fragile to hold its continent-wide trade empire together. George Simpson, a young Scotsman, had spent one year commanding the Hudson's Bay Company's operations in the Athabasca country. Under the terms of the union, Simpson assumed command of the Northern Department. His tact and diplomacy assured the success of the unification.

Between 1821 and 1839, the Hudson's Bay Company, under Simpson's guidance, devoted much energy toward exploring in the direction of the Pacific slope and intercepting the furs traded to the British, Americans, and Russians along the Northwest Coast. Outflanking the competition, however, was not the sole motive in the company's exploration of the Far Northwest. In fact, expansion of the fur trade and exploration had gone hand in hand from the earliest days, carried forward by such explorers as Samuel de Champlain, Pierre Esprit Radisson, and Medard Chouart des Groseilliers, to name but a few. Another factor impelling exploration was the desire of the traders to avoid the Indian middleman's tariff on furs. Expansion enabled traders to establish direct contact with the Indian trappers, thereby eliminating the middleman. Karamanski cites a third reason for exploration: the expanding geographic perception of North America. Captain James Cook, Peter Pond, and Sir Alexander Mackenzie all had suggested the possibility of a great river valley existing west of the northern Rocky Mountains, and the Hudson's Bay Company had fallen heir to America's last geographic puzzle.

The author states that the literature often describes the period between 1821 and 1850 as one of retrenchment for the Hudson's Bay Company. Internally, the company attempted to economize and establish a monopoly, but economically and diplomatically it struggled with American interests over the ownership of the Oregon Territory. The author brings these policies into sharper focus as he describes events in the Far Northwest. The Hudson's Bay Company combined policies of economy and conservation of fur supplies with a program that

included exploration and expansion along the undeveloped frontiers of the Far Northwest. George Simpson, nicknamed "the little Emperor" by his foes (p. xvi), conceived and directed this strategy. He spurred company personnel to explore and expand, but withheld the funding and personnel necessary for these tasks. In fact, his limitations as an administrator eventually resulted in the company's failure to expand into the upper Yukon country.

The author maintains, and shows, that the explorers were ordinary men. Having traveled widely in the region he deals with, Karamanski is able to discuss the attitudes, actions, and adventures of these men with sympathy and understanding. Modern readers will have difficulties comprehending the hardships these men endured in the line of duty. Robert Campbell and his group, for example, wintered at Dease Lake. They starved, and as spring came, his men "tore off the webbing of their snowshoes and boiled it up for dinner. Their last meal at Dease Lake was the boiled parchment of the post's windows, which had the savory consistency of glue" (p. 153).

In short, this is a good book. The author ably integrates the complicated diplomatic affairs of the British, Americans, and Russians into his narrative. He describes the exploration of an area still relatively unknown today. He includes a useful bibliography, listing the important materials available on the region. One very minor matter—it was not the Russian American Fur company (p. 24) but simply the Russian-American Company.

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LAUREL SEFTON MACDOWELL. *"Remember Kirkland Lake": The History and Effects of the Kirkland Lake Gold Miners' Strike, 1941–42.* (The State and Economic Life, number 5.) Buffalo: University of Toronto Press. 1983. Pp. xvi, 292. Cloth \$30.00, paper \$12.50.

Remember Kirkland Lake? Canadian workers in the early years of the Second World War could think of little else. It was the site of their most momentous battle against the combined forces of capital and government. It was also the site of their greatest defeat. Yet, as Laurel Sefton MacDowell makes clear in this book, out of the ashes of this catastrophe, Canadian workers achieved their most important triumph.

On November 18, 1941, some thirty-seven hundred miners, members of local 240 of the International Union of Mine, Mill, and Smelter Workers, walked off their jobs in the eight gold mines in

Kirkland Lake, a bustling gold town some five hundred miles north of Toronto. The mine operators, fully supported by a reactionary Ontario government, refused to negotiate with the union. Despite the findings of a government-supported conciliation board that favored the union, the mine owners remained adamant. They would never deal with a CIO union.

Most Canadian labor leaders opposed the strike. They feared that it would cost their newly created industrial union movement much in terms of funds, effort, and credibility, with little to be gained in return. Yet the miners in Kirkland Lake had no choice—and no chance. All the cards were in the hands of the owners. Expecting a strike, they had large stockpiles of ore, and had at their beck and call the Ontario Provincial Police. Above all, the owners had the weather on their side. Midwinter strikes in frozen northern Ontario are not notably successful.

By February the strikers were forced back to work—at least those the mine companies would take back. Union activists could look for work elsewhere—or join the army. In the words of one participant: "The Strike was pretty well disastrous. We lost our jobs, our homes, everything."

Yet as MacDowell makes clear not everything was lost. Indeed, she sees the strike as a turning point in the history of the Canadian labor movement. It proved to be a valuable training ground for a dedicated cadre of labor leaders—including the author's father—who would change the face of Canadian unionism over the next three decades. Above all, it united the labor movement and drew public attention to the nefarious antilabor policies of the Canadian government. It produced much public pressure—from the press, the pulpit, and Parliament itself—that compelled a hesitant federal cabinet to give Canadian workers what they wanted, and what they had struck for at Kirkland Lake; a Wagner Act. Privy Council Order 1003, the so-called Magna Charta of Canadian labor, was, in MacDowell's view, a direct result of the Kirkland Lake strike.

Although the book lacks a social history context—one would desperately like to have heard more from, and about, the strikers themselves—this is a worthy study of one of the key events in the history of the Canadian labor movement.

IRVING ABELLA
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JAMES STRUTHERS. *No Fault of Their Own: Unemployment and the Canadian Welfare State, 1914–1941.* (The State and Economic Life, number 6.) Toronto: University of Toronto Press. 1983. Pp. x, 268. Cloth \$30.00, paper \$12.50.

James Struthers has written an important contribution not only to Canadian history, but to the comparative study of the growth of the welfare state as an aspect of public policy. He begins his study of the treatment of unemployment at the beginning of World War I, and ends with the passage of an unemployment insurance act in the early stages of World War II. In between is a detailed account of how the various levels of the Canadian federal state sought to avoid responsibility for the terrible human problem of mass unemployment during the Great Depression. It is not an edifying story, although well told.

As Struthers makes clear, Canada had special problems: high seasonal unemployment, a staples export economy at the mercy of world demand, and an agricultural frontier in the prairies that gave rural interests a leverage over policy unmatched in more industrialized nations. Behind these factors was a rather simple-minded, laissez-faire ideology that treated unemployment as the result of laziness and lack of character—an ideology little changed from that which animated the British poor law reforms of the 1830s.

Furthermore, the logic of the marketplace demanded a reserve army of labor. The result was a disastrous series of expedients to stem the specter of social disorder glimpsed in the angry faces of the jobless. Relief camps for single unemployed men (failures in stark contrast to the New Deal's CCC), anachronistic "back to the land" schemes, vicious local crackdowns on the dole, and endless squabbling between local governments trying to shunt the jobless out of their own jurisdiction were the fruits of the dominant ideology.

Neither of Canada's major political parties was capable of matching even the public relations flair of the New Deal, let alone the experimental ideas thrown out by the Roosevelt administration. Ironically, there were elements of Canadian business that appeared at times to be more daring than the government in their approach to the unemployed. Moreover, the sporadic efforts made by social workers to push for professionalization, only strengthened the government's determination to exercise more control and regulation over the "shiftless" poor.

In the end, it was war that finally led the Canadian government to cast aside the crutch of the constitution and to institute a national unemployment insurance scheme. It was the war and the national economic responsibilities it forced on the federal government that finally fixed responsibility for the jobless with the state. *No Fault of Their Own* is an excellent contribution to the literature that has debunked the role of altruism in the growth of the welfare state.

REGINALD WHITAKER
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HARVEY G. SIMMONS. *From Asylum to Welfare*. Downsview, Canada: National Institute on Mental Retardation. 1982. Pp. xv, 355. \$24.00.

The history of psychiatry, and particularly of asylums, has attracted substantial scholarly interest over the past decade. A comparatively neglected area within this larger field is the history of mental retardation. Harvey G. Simmons, a political scientist, attempts to correct this deficit in the Canadian context by tracing mental retardation policy from its almost imperceptible origins in the asylum movement of the 1840s to its place in contemporary social welfare programs.

Public policy, Simmons argues, is neither fortuitous nor the result of vague nonhuman agency. Although he concedes that both structural variables and political ideology may shape general attitudes, specific lines of policy are determined by competing interest groups. Since no single group is predominant, the resulting policy assumes over time a deceptively incremental or adventitious quality. In the case of the mentally retarded, three major groups influenced policy: elected officials and bureaucrats, social reformers and physicians, and patients and their families. Each faction elaborated positions on issues such as the provision of special education facilities, the quality and availability of institutional care, and the mechanisms necessary for deinstitutionalization. Depending on the interplay of these groups, certain themes characterize specific chronological periods. During the nineteenth century, the retarded were accorded no unique identity, but were consigned by bureaucrats to obscure corners of large psychiatric hospitals. In the early years of the twentieth century, in part at the instigation of medical degeneration theorists, the retarded were perceived as a dynamic social threat for whom an aggressive policy of institutional confinement was essential. During the Depression and Second World War, the retarded lost their menacing aura and again fell victim to bureaucratic neglect. Only with the birth of organized parental pressure groups in the 1950s did the retarded assume a distinct identity. The result was a vigorous deinstitutionalization campaign designed to place individuals in the community and to normalize their education, employment, and personal relationships.

Although Simmons has written an informed and detailed study of public policy, not all aspects will appeal to social historians. He has ignored dissertation literature dealing with the insane asylum in nineteenth-century Canada and his grasp of asylum historiography is occasionally unorthodox. Is it accurate, for example, to suggest that David Rothman is "usually so careful to avoid taking a social control approach" (p. 44)? Since Simmons views policy formation as a deliberate and rational exercise, he

refuses to impute covert motivation or to accord a causal role to historical forces beyond the control of policy makers. Such analysis, with empirical support, is the essence of the historian's task. By refusing to accept this challenge, Simmons becomes distracted by detailed discussions of the motives, humanitarian or sinister, of individual players, to the exclusion of more relevant macro-level generalization. Finally, in an almost Whiggish sense, the book accepts for the nineteenth century what Christopher Lasch has called the "reform to custody" formula and views subsequent moves away from custodialism as a clear indication of human progress. A brief glance at recent critiques of the decarceration movement casts this contemporary optimism in a much less sanguine light.

Despite such criticisms, Simmons has produced an impressively documented volume that not only creates a coherent chronology of events, characters, and issues, but also astutely relates Canadian developments to themes in the wider Anglo-American community. It is clearly an important contribution to the social and political history of Canada since 1840.

S. E. D. SHORTT
Queen's University

EDWARD MCWHINNEY. *Canada and the Constitution, 1979–1982: Patriation and the Charter of Rights*. Buffalo: University of Toronto Press, 1982. Pp. xii, 227. Cloth \$29.50, paper \$10.95.

As a drama, Canada's recent patriation of its constitution was marked by improvisation and, it sometimes seemed, by influences from the theater of the absurd. In the end, most of the players, with the exception of Quebec's René Lévesque, were satisfied with the outcome. Most Canadians, however, have not yet rendered their judgment on the protracted constitutional squabble and the constitutional changes it produced. Edward McWhinney's book is an early and successful attempt to clarify what happened between 1979 and 1982 and to predict what the constitutional changes will mean for Canadians.

The overall impression of the constitutional struggle presented in this book is one of a failed opportunity. The first mistake, in McWhinney's view, was Prime Minister Trudeau's reluctance to abandon a conservative and traditional approach to the reform of the Canadian constitution. Rather than bringing the people into the constitutional process through a stirring declaration of rights and a subsequent plebiscite, Trudeau opted for a legalistic and prosaic approach that used the British Parliament to certify Canadian changes. The loss in this approach was twofold. Canadians as a whole were spectators and

not participants. Perhaps a more serious consequence was the opportunity that this legalism gave to the provincial premiers. With the exception of William Davis of Ontario and Richard Hatfield of New Brunswick, the premiers vigorously opposed Trudeau's constitutional plans, arguing in the courts and in the political arena that the proposed constitutional changes were an assault on the fundamental freedoms of Canadians.

McWhinney believes that it was the premiers who managed to limit the rights of all Canadians by emphasizing provincial rights. They watered down the initial charter that Trudeau presented, especially through the additions of the so-called "not withstanding" clauses that permit provinces to opt out of several parts of the Charter of Rights and Freedoms. The premiers, McWhinney strongly argues, constituted an illegitimate body in the constitutional process that did not represent the opinions and feelings of Canadians. He further believes that the best hope for the future is a constitutional process in which Canadian cities, among others, will take an active part thereby limiting the influence of the provincial premiers.

It is hardly surprising that McWhinney is disturbed by the premiers' tactics, such as bargaining support for human rights in exchange for higher oil prices. Nevertheless, he is probably incorrect to suggest that the premiers could have been shunted aside in the constitutional debate between 1979 and 1982. For better or worse, the federal-provincial conference has become a Canadian political institution that could not have been suddenly abandoned. Nor does the historical record suggest that a plebiscite would have been particularly useful. The federal government was probably wise to reject its use.

Canada and the Constitution is a valuable book based on McWhinney's exceptional background in constitutional law and his access to private sources. No historian of contemporary Canada can afford to ignore it.

JOHN ENGLISH
University of Waterloo

LATIN AMERICA

GEORGE A. COLLIER *et al.*, editors. *The Inca and Aztec States, 1400–1800: Anthropology and History*. (Studies in Anthropology.) New York: Academic Press, 1982. Pp. xx, 475. \$47.00.

This collection of ethnohistorical articles on the Aztec and Inca empires, 1400–1800, edited by George A. Collier, Renato I. Rosaldo, and John D. Wirth, will prove to be richly rewarding for historians and anthropologists alike. Although common

themes for the volume are well established, not surprisingly for Latin Americanists, the contributors do not always agree. For example, there are a variety of responses to the question: how integrated, centralized, and tightly controlled were the two empires? Pedro Carrasco's highly general comparative piece immediately strays from conventional wisdom by arguing that the political economies of the Incas and Aztecs were remarkably similar. With regard to the Inca empire, John Rowe and Catherine Julien demonstrate that imperial labor policies and the decimal system of organization effectively destroyed regional loyalties and created new imperial attachments. Yet John Murra, Franklin Pease, and Craig Morris show that there was far less centralization and integration than generally supposed. Nathan Wachtel is not quite sure. With regard to the Aztec empire, however, Edward E. Calnek and J. Rounds both emphasize the regional autonomy of subject peoples, Calnek stressing the rise of the city-state system and Rounds the corporate sharing of power based on the fraternal line of succession and "the lowered threshold of legitimacy" (p. 83).

Concerning the effects of Spanish rule there is at least some agreement. Woodrow Borah's study of Spanish and Indian law in New Spain shows that by 1821 these laws and their application left the Indian as "the wretched of the earth" (p. 285). With regard to Peru, Steve Stern points out that Spanish institutions in Huamanga, even those designed to protect the Indian, created conflicts within the indigenous society and strengthened the hand of the Spaniards. In the same vein Karen Spalding sees the colonial economy as a unified rather than a dual system. Held together by the exploitation of Indian labor and the preservation of Indian communities, this system prevented the formation of a competitive labor market. The Indians did, however, find means to counter imposition of Spanish rule. Jorge Klor de Alva shows that the Indians in Mexico effectively resisted spiritual conquest and devised a variety of ways to maintain indigenous religious beliefs and practices. James Lockhart's sophisticated analysis of Mexican village land titles also demonstrates retention of a remarkable degree of corporate identity and cultural continuity. Frances Karttunen shows that the Nahuatl language and literacy did not survive and had virtually collapsed by the end of the colonial period. In the last essay, Tom Zuidema ignores the Spanish presence in Peru entirely to postulate the Inca's ability, without the benefit of written language, to develop systematic, cumulative knowledge and particularly abstract ideas of time and space.

This collection demonstrates both the strengths and weaknesses of the ethnohistorical approach. On the positive side one is struck by the ingenious use of

evidence—*visita* reports, *quipu* counts, archeological findings, linguistic and ritual evidence found in land titles, and Inca sightlines in Cuzco are just some examples. Also, more traditional historical pieces clearly show the advantages of the ethnohistorical approach in turning conventional history away from simple narrative and self-righteous moral judgment toward the more fruitful realm of moral criticism. Still, some problems remain. Despite the heroic efforts of George Collier (Introduction) and Renato Rosaldo (Afterword) to bring cohesion to the essays, many remain disparate, discipline-bound, and non-comparative. The subimperial approach, as Collier terms it, has often resulted in idiosyncratic studies of one particular aspect of Inca or Aztec life without any attempt at comparison or generalization. On balance, however, these essays are almost uniformly of high quality. They are instructive in their innovative use of evidence, their penchant for taking a fresh look at old and new problems, their sallies into the realm of moral criticism, and their focus on the mentalities and day-to-day functioning of elemental society.

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GERALD CARDOSO. *Negro Slavery in the Sugar Plantations of Veracruz and Pernambuco, 1550–1680*. Washington: University Press of America. 1983. Pp. xi, 211. Cloth \$21.50, paper \$10.75.

Gerald Cardoso's book is a pioneer effort and as such is worthy of attention. Too few writers have attempted to apply comparative methods to the study of the social history of colonial Latin America, and that is one reason why our knowledge of that phase of human experience is still a great deal less useful to both theory and politics than it might be. For purposes of systematic comparison, the sugar plantation regions of Mexico and Brazil in the sixteenth and seventeenth centuries are a natural choice. Both relied initially on an Indian labor force that did not withstand for long the double onslaught of the Old World's epidemic diseases and system of forced labor in commercial agriculture. Both areas then replenished their labor forces from West Africa. Slaves in the two colonies were employed in the same production with the same technology for the profit of a few European settlers and their Iberian monarchs. Moreover, because plantation sugar production is the most thoroughly studied labor system of the colonial Americas, by this time the possibilities for a sophisticated essay in the social history of any sugar-producing area are considerable.

What might have given the researcher pause about this undertaking at the outset, however, is the

great discrepancy between the materials available on each region. Since colonial Pernambuco is a core subject in the early history of Brazil, it may be studied in a number of contemporary chronicles and published collections of documents as well as numerous monographs on this region. In contrast, Veracruz has been dealt with only peripherally in the historiography of early Mexico. This discrepancy might have been compensated through the discovery of some rich lode of primary source material on sugar-producing society in Veracruz, but lacking such material, a fruitful comparison is difficult to contrive.

Cardoso reviews the histories of the two sugar-producing slave societies during the better part of the sixteenth and seventeenth centuries in parallel fashion. In each case he traces as well as he can the Indian demographic catastrophe and the administrative and demographic history of African forced settlement. He then attempts to reconstruct the labor regime for African slaves, the social and cultural life of slave communities, and the history of slave resistance. The conclusion seeks to make explicit the comparisons the author has found instructive.

The book will be of some use to students as an introduction in English to slavery in Veracruz and Pernambuco, but it is far from providing a thorough synthesis of the subject. Scholars of colonial Latin American history will find it disappointing. Despite the scattering of references to archival material in five countries, the author's treatment of both regions is based primarily on standard monographs and fails notably to improve on them. The use of sources and of the comparative literature on slave society is uncritical, and unsubstantiated assertions abound. The discussion is uninformed by social science of any kind (though several anthropologists are cited) or by the many available studies of slave sugar production in the West Indies. The sketches of slave society in Veracruz and of Indian society in Pernambuco are pulled together primarily and without apology from evidence drawn from other areas, yet the examination of slave society in Pernambuco derives no benefit from the rich historiography of nearby slave Bahia. The result is that both societies emerge as unevenly drawn archetypes rather than as closely reconstructed, particular historical experiences suitable for comparison. Most critical in the last analysis is that the "comparison" turns out to be nothing of the kind—but rather an exercise of dubious utility in the artificial adaptation of standard images of slave society to two differing contexts. The typographical standard of the book's publisher also leaves much to be desired.

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MICHAEL CRATON. *Testing the Chains: Resistance to Slavery in the British West Indies*. Ithaca: Cornell University Press. 1982. Pp. 389. \$29.50.

This is an important book. Rather surprisingly, it is the first comprehensive account of slave resistance and revolts in the British Caribbean. It is therefore to be highly welcomed.

Michael Craton first discusses resistance short of rebellion. He rightly suggests that the ways in which slaves fashioned a life of their own and thereby shaped plantation society "constituted the most fundamental form of resistance" (p. 33). Equally important was the interdependence of white and black. This was highlighted by Lady Nugent, the wife of an early nineteenth-century governor of Jamaica, who noted that creole (local-born) whites spoke and behaved like their slaves, not vice versa. Other forms of resistance included the survival of African culture, the development of strong family ties, and running away. But Craton concludes that most cases of resistance short of rebellion were acts of noncooperation.

The core of *Testing the Chains* is devoted to plots and rebellions from 1600 to 1832. Craton reviews the history of the Jamaican Maroons and the African phase of resistance more generally. He provides a useful critique of Eugene Genovese's concept of "restorationist" rebellions led by Africans. Although there was an element of this in the early rebellions, Craton carefully demonstrates the role of creole slaves in the plots and outbreaks in Barbados in 1692, Antigua in 1736, and Jamaica in 1760. The Antigua rebellion was particularly significant: one-third of the participants were African, but the rest were creole. There were also crucial differences between the two groups of plotters. The Africans wanted a war of extermination against the whites, while the creoles envisaged a creole commonwealth in which Africans would continue to be enslaved.

Another theme of the book emerges in Craton's analysis of Tacky's Revolt in Jamaica in 1760. This massive outbreak was dominated by Africans (Coromantees), yet while some slaves rebelled, others were armed to protect their masters. The revolt demonstrated that blacks (including Maroons) would be crucial in helping whites crush black rebellion.

Plots and rebellions were not confined to the larger islands or colonies. The Maroons of Dominica, the Black Caribs of St. Vincent, and the Africans in Tobago all threatened white hegemony. In some islands, such as Grenada in 1796, slaves nearly established a black republic. It took enormous British forces to put down these revolts—with devastating results for soldiers and rebels. For some of the rebels, such as the Trelawny Maroons of Jamaica

and the Black Caribs, it meant the deportation of entire communities.

Despite the planters' hopes, an increasingly creole slave population did not mean fewer revolts. All that changed, according to Craton, were the forms of resistance. The Age of Revolution thus witnessed increasing resistance rather than increasing accommodation. Again challenging Genovese, Craton maintains that the post-1775 outbreaks were not the result of the liberal ideas of the period; instead, they arose from the usual effects of overwork, dislocation, and continuing African majorities. The causes of slave unrest, then, were essentially internal and indigenous.

Craton argues that in the last phase of slave resistance, 1816–32, slaves helped emancipate themselves. They influenced metropolitan ideas and reshaped the Christianity of the European missionaries. The privileged slaves, often the most discontented, played on rumors of freedom to instigate revolts. Yet the slaves' vision of freedom did not necessarily mean taking over the estates; rather, it was the freedom of an independent peasantry, a "proto-peasantry" in Craton's terms. But what stands out in these later rebellions (Barbados in 1816, Demerara in 1823, Jamaica in 1831–32) as well as in the earlier plots and conspiracies, is the slaves' will to resist. As the leader of the Jamaican rebellion in 1831–32 put it before being hung: "I would rather die upon yonder gallows than live in slavery."

Testing the Chains is full of useful insights, yet there are some problems with the book. For example, the role of ideas in slave resistance requires much more investigation. We do not yet know whether radical ideas or the disruptions caused by the Age of Revolution were of greater importance. In addition, there are some curious omissions in the references. The important works of Seymour Drescher on slave economies, Edward Cox on Grenada, and Hilary Beckles on Barbados are absent. We also need to know more about the other forms of nonviolent resistance: day-to-day resistance, sabotage, and running away. Craton's attempt in an epilogue to link the late slave rebellions with the postemancipation riots and rebellions in Demerara (1856), Jamaica (1865), and Barbados (1876) needs to be more carefully thought out.

Still, *Testing the Chains* is a pioneering study on a vitally important topic. It is a significant addition to Caribbean historiography. Like most pioneering studies, however, it will help start the debate rather than conclude it.

GAD HEUMAN
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BONHAM C. RICHARDSON. *Caribbean Migrants: Environment and Human Survival on St. Kitts and Nevis*.

Knoxville: University of Tennessee Press. 1983. Pp. xiii, 209. Cloth \$19.95, paper \$12.50.

This work focuses on migrations from two small Caribbean islands with a combined total population in 1970 of less than 45,000. Bonham C. Richardson views this potentially narrow subject with an admirably broad perspective, examining 150 years of Kittitian and Nevisian migration in historical, geographical, economic, sociological, political, and ecological terms.

Labor migration is a fundamental element in contemporary Kittitian and Nevisian life. The roots of this migratory tradition lie in the 1830s, the period of emancipation in St. Kitts and Nevis. Facing exploitative conditions on the English-owned plantations, freed slaves soon began to explore opportunities away from their home islands, beginning with locations to the south, particularly Trinidad. When these migrants returned with savings and reports of relatively better working conditions, others followed, establishing a pattern that persists to this day. For a long time, migrants limited their travels to the Caribbean, but in the twentieth century they also headed to Great Britain, Canada, and the United States (the latter two attracting much of the islands' small middle class since 1950). The author identifies four periods of heavy migration from St. Kitts and Nevis: first, the early movement to Trinidad; second, migration to Bermuda from 1900 to 1905; third, travel to the Dominican Republic between 1900 and 1930; and fourth, the mass migration to Great Britain in the 1950s and early 1960s.

This book is more than a description of migrants and migration. Richardson wants the reader to understand the experience behind migration. Thus he develops lengthy and quite useful sections on the Kittitian and Nevisian slave experience, abolition, the plantation economy, the ecology of sugar and peasant agriculture, and the nature of migrant communities abroad. He also evaluates Kittitian and Nevisian experiences in light of the rest of the Caribbean. Caribbeanists will note many close parallels.

Richardson seeks to understand migration in a local context and, more importantly, from the point of view of the migrants themselves. Larger theories (which he outlines in relation to his own work) are fine, he argues, but by themselves they often fail to elucidate what is most important about a particular situation.

Richardson identifies early Kittitian and Nevisian migration as a "postslavery migration adaptation," adopted because nearly all land was controlled by the planter elite, leaving no possibility of independent farming. Although in the case of Nevis, the situation changed over the next century as the

inefficient sugar industry of Nevis (but not of St. Kitts) collapsed, the land over which the Nevisians gained control was so destroyed by several centuries of colonial exploitation that it produced little. He adds that today overuse is further ruining the land.

Given the meager resources of their home islands, migration has represented an alternative means of survival to Kittitians and Nevisians. Migration-produced income is an important economic resource, both for the migrants themselves and for the kin they leave behind. Migration also has an impact on the home culture, for it increases the migrants' status, influences the nature of the family, encourages individuality and mobility (thus limiting political and labor organization) and promotes flexibility and innovation (thus discouraging economic specialization).

Richardson's work is broad, well researched, and well argued. It is also generally well written, although there are some passages that would have benefited from an editor's greater attentiveness to flaws such as repetition.

BRUCE CALDER
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ROBERT MCCAA. *Marriage and Fertility in Chile: Demographic Turning Points in the Petorca Valley, 1840–1976*. (Dellplain Latin American Studies, number 14.) Boulder, Colo.: Westview. 1983. Pp. xv, 207. \$20.00.

About thirteen years ago Robert McCaa began to copy from parish and civil registries in the tiny Chilean town of Petorca the vital statistics of its population over the past two centuries. The thousands of hours spent in gathering and ordering this information, the sorting and elimination of incomplete material, and the frustrations of ambiguous evidence are all recorded in the appendix of his recently published book. Even though my memory of McCaa—first in Santiago where he was barely visible among the notes, and later in Cambridge where cards and tapes spilled out of his pantry into the stairwell—is not faint, reading the story of his labor makes my own arms droop with fatigue. McCaa has produced a narrow monograph of some two hundred pages on an obscure village at the end of the earth; discernible between the lines is the story of how McCaa trained himself to be perhaps the most acute historical demographer now working on Latin America.

The author's aim is to explain changes in population through an examination of fertility and mortality using the methods of family reconstitution developed by Louis Henry. The method requires the elaboration of family genealogies from data on

births, deaths, and marriages. Because such a procedure is enormously time-consuming, only a small number of families can be dealt with, and, of these, many must be eliminated because they move out of the community or otherwise statistically disappear. This leaves only a small number of families that tend to be rural, illiterate, and unadventurous; consequently, it is difficult to project local practice to a national level. No one knows this better than McCaa. His comments on bias and limitations in his work and his informed discussion of the French and Cambridge groups are two useful features of the book.

McCaa has made an effort to ground his demographic study in social and historical reality. There is a good chapter on the social geography of Petorca and a lively discussion of celibacy, coupling, and marriage that sets out in detail the salacious activity of the hard-bitten miners and their extraordinarily fertile companions. The author is informed about how people make their living; their position within the larger movements of Chilean history; the nature of class structure; and sources of communication. McCaa also includes a section on interracial coupling, a subject that he is now pursuing in recent work on eighteenth-century Mexico. Much of this material is unusual, interesting, and valuable.

Although McCaa necessarily qualifies his conclusion, he does suggest an interesting variation on the classic demographic transition theory. As mortality rates dropped after 1930 and especially after 1960, the people of Petorca rapidly began to limit family size so that within a single decade fertility plummeted to rates comparable to those in the present-day United States. This transition in the Petorca valley was not accompanied by political coercion or substantial economic growth but, rather, by a sea change in values and attitudes induced by the outside—through national television and press from Santiago—together with artificial contraception. More than anything else, fertility decline correlates with occupation and literacy. It is a pattern of behavior that spreads from urban elites to the rural masses. Thus, a modern demographic regime has come into existence in the Petorca valley without the economic and social development typical of European communities at a similar demographic stage. The Chilean example demonstrates, in McCaa's opinion, "how far a country may go in achieving a modern demographic structure without escaping the bonds of dependency, underdevelopment and social inequality" (p. 149).

McCaa's pioneering monograph is both a field-worker's manual and a local demographic history. It is rich in detail, theoretically sound, and, I believe, the first study of its kind on a Latin American community. A certain number of specialists will be interested not only in the conclusions but also in the

way McCaa arrives at them, which is amply discussed in a kind of methodological dialogue the author carries on with himself throughout the book.

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RICHARD GILLESPIE, *Soldiers of Perón: Argentina's Montoneros*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xvi, 310. \$39.95.

This brilliant and incisive study of Argentina's Montoneros, the most powerful guerrilla organization of the 1970s, establishes Richard Gillespie as a major interpreter of that nation's political conundrum. It is Gillespie's purpose and achievement to examine the Montoneros neither as heroic freedom fighters nor as bloodthirsty terrorists, but objectively and critically. Based on a solid foundation of primary sources and interviews, this book is a success from start to finish.

The volume is divided into six chapters. The first two chapters deal with the historical and ideological background of the movement, displaying Gillespie's keen knowledge of the Argentine past. Chapters 3 through 6 analyze the development of the Montoneros in four distinct stages: the last three years of the 1966–73 military regime; the 1973–74 presidencies of Héctor Campora and Juan Domingo Perón; the unfortunate 1974–76 administration of María Estela Martínez de Perón, the first female president of Argentina; and, finally, the first five years of the military regime installed in March 1976 and initially headed by General Jorge Rafael Videla.

First organized during the years 1968–70, the Montoneros were young, urban, middle-class, Catholic, radical nationalist, and socialist. They identified with the new Peronist left. In the years 1970–73, through selective kidnappings and terror, they strode successfully across the Argentine stage. The Montoneros helped bring down the brief military regime of General Roberto Levingston and applied pressure on his successor, General Alejandro Lanusse, for elections in 1973 and the return of Perón.

But the Montoneros were guilty of naiveté on two major grounds. They believed in the rhetoric and not the record of Perón—the *caudillo* who first used them and later betrayed them when he took power. Furthermore, the Montoneros failed to recognize that their insurrectionist militarism was alien to the orthodox Peronists and the “economism” of the Peronist masses, conditioned for decades to pork-chops trade unionism. Hence, it was not only the “draconian authoritarianism” of the Argentine military, but also the indiscriminate terrorism of the Montoneros in and after 1975 that brought about their downfall a few years later.

This volume should be read by social scientists of all disciplines, for it punctures many of the simplistic concepts of transcultural generalization. First, Peronism was not originally a variant of fascism but represented a deeply rooted *caudillismo* reaching out to the masses for legitimation. Second, political change occurs not only when the nationalistic middle class stirs the masses against the cosmopolitan oligarchy, but, as in the case of the Montoneros, when a sector of the middle class turns against its own stratum. Finally, Marxist historians, beware! For Gillespie demonstrates that the working class of Argentina is hardly revolutionary and is imbued instead with a conservative “economism.”

It is clear, then, that Gillespie's book is not only an excellent case study of urban guerrilla warfare; indeed, its relevance transcends the frontiers of Argentina.

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MICHAEL L. CONNIFF, editor. *Latin American Populism in Comparative Perspective*. Albuquerque: University of New Mexico Press. 1982. Pp. xiii, 248.

Is populism alive or dead in Latin America? Does Latin American populism still serve as a viable transitional political formula in the hiatus between the decline of traditional structures and the rise of new ones? Or has populism's “moment” already been superceded? These are the questions that editor Michael L. Conniff and his collaborators wrestle with in this stimulating volume.

Populism, according to Conniff, was a major force in Latin American politics from the 1920s to the 1960s. The rise of populism was intimately connected with urbanization and the emergence of working-class mass movements. Populism was thus an agent of change (albeit limited) that secured for both workers and the middle classes political participation, democratic rights, unionization, and new social programs. Yet populism seemed to give way in the 1970s as unfulfilled expectations provoked social unrest that precipitated a new wave of authoritarian regimes.

There are good essays by David Tamarin on the Argentine populism of Yrigoyen and Perón, Marysa Navarro on Evita, Conniff on Brazil under Vargas, Jorge Basurto on Mexico's Luis Echevarría, Steve Stein on Peru's APRA, and Steven Ellner on Betancourt and Acción Democrática from 1935 through 1948. The thrust of these essays is that, although populist leaders like Perón, Vargas, or Haya de la Torre may have at times sounded like revolutionaries, in fact, they sought progress without upheaval. They tried to provide political, social, and economic benefits to the new urban masses (which is editor

Conniff's definition of populism), but they did so without fundamentally changing society. These arguments seem both provocative and defensible.

The book purports to treat populism in comparative perspective, but that effort does not quite succeed. The essays on populism in the United States and in pre-revolutionary Russia fail to bolster the argument and are not well integrated with the rest of the book. A genuinely comparative and integrating essay that linked Latin American populism with, let us say, Poujadism in France, the phenomenon of Mussolini in Italy, populist leaders in other Third World areas, or other regimes and movements would have been useful. But that was not attempted.

Moreover, it is not certain that populism has run its course. The editor's own definition of populism and his description of how and when it emerges points toward the possibility of its renewal or persistence in Latin America. These possibilities are hinted at strongly in both the intriguing foreword by John D. Wirth and the excellent conclusion by Paul W. Drake. The authoritarian military regimes that provide the context for this book and seem to signal populism's passing are, in fact, giving way in a number of countries to more democratic tendencies. Therefore, the conditions and possibilities for other forms of populism, both civilian and military, still seem to be present. Conniff's path-breaking work provides a theory of and comparative perspective on Latin American populism and enables us to comprehend better these recent developments.

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SERGIO DÍAZ-BRIQUETS. *The Health Revolution in Cuba*. (Special Publication, Institute of Latin American Studies.) Austin: University of Texas Press. 1983. Pp. xvii, 227. \$19.95.

The ambitious title of this work leads one to expect a much-needed history of Cuba's health improvements, probably the greatest social achievement of Castro's revolution. Instead, this revised doctoral dissertation would be more appropriately entitled "A History of Mortality Decline in Twentieth-Century Cuba." This in itself is a significant contribution but far from the complete history that is yet to be written.

Sergio Díaz-Briquets traces mortality decline through different historical periods and examines the reasons for increased life expectancy and declines in cause-specific mortality since Independence. The Cuban-born demographer presents his

data skillfully within the context of important historical developments, and he demonstrates cogently the wide variety of socioeconomic, political, and scientific factors contributing to increased life expectancy. He is particularly meticulous in his attention to the quality of his data and its limitations.

The main disappointment in this work is its brevity. Subtracting a lengthy appendix that includes evaluations of data availability and quality, explanations for calculations, and scores of charts, graphs, and statistical tables, the reader is left with fewer than one hundred pages of text. That may be sufficient to nourish demographers, but historians will be left hungering for more. This brief analysis for all of twentieth-century Cuba includes scarcely a dozen pages on the period since Castro, thus leaving many unanswered questions.

Despite the section's brevity, Díaz-Briquets presents a balanced view of the controversial post-1959 period. He demonstrates that improvements in socioeconomic justice and equity along with expanded public health and medical services have made Cuba's life expectancy one of the highest in the developing world. His data serves as a corrective to the evaluations of those (mostly exiles) who have depreciated Cuba's health achievements. At the same time, the author chastens those who have exaggerated post-1959 accomplishments by demonstrating the extent to which mortality had declined before the revolution. The author's sophisticated understanding of demography and public health issues probably accounts for the differences between his conclusions and less optimistic observations in works like Lowry Nelson's *Cuba: The Measure of a Revolution* (1972).

The author's insufficiently developed comments on the excessive social and political costs of Cuba's changes and the limited applicability of its health revolution to other poor countries will surely draw criticism. Brevity again is the culprit, for one cannot appreciate Cuba's accomplishments without comparing the enormous inequities and injustices characteristic of health systems in other developing countries, even wealthier ones like Mexico and Brazil.

Díaz-Briquets recognizes the limitations of using conventional economic indicators in assessing Cuban progress. Yet he slips into that web by using data such as decreased per capita income to explain his view that economic factors are less important in contemporary health improvements in Cuba. Indicators like per capita income lack significance in evaluating topics such as nutrition in the face of increased equity, free meals in day care centers, schools, and factories, and other social changes that affect health but fail to fall within "conventional" economic analysis. Such subjects deserve more attention.

In short, this is an excellent collection of mortality data with a useful, if brief, analysis. It is the most complete study we have of mortality transition in a developing country, but it is far from a history of the politics and process of Cuba's health revolution.

More attention to the latter would have made a good book a superb one.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

GEORGES LIVET and BERNARD VOGLER, editors. *Pouvoir, ville et société en Europe, 1650–1750*. (Colloque International du C.N.R.S., 1981.) Paris: Editions Ophrys, for the Association des Publications près les Universités de Strasbourg. 1983. Pp. xiv, 627. 230 fr.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

Nikki R. Keddie has made an important contribution to our thinking about the Iranian Revolution ("Iranian Revolutions in Comparative Perspective," *AHR*, 88 [1983]: 579-98). There are some points, more of emphasis I would like to make. Iran bears a relationship to Western society and the forces of modernization strikingly similar to Russia in the nineteenth and early twentieth centuries.

The modernizers are often seen both as traitors to a national native culture and as leaders of nationalism and national revival through reform. This leaves their leadership subject to all sorts of ups and downs and uncertainty.

Another important consideration in Iran (and throughout the Third World) is the staggering effects of urbanization. It is interesting that Trotsky and others had noted the instability created in the old regime in Russia by the large influx of peasant workers brought about in Czarist Russia by the war and the growing urban instability that resulted. This same situation was produced in Iran by both Pahlavi shahs through wanton disregard for agriculture (until too late) and intense concerns with industrial and urban development.

The tragic fact is that there has long been a political base for modernization and with moderation in Iran and this has been neglected by the United States. My father's two missions to Iran in the 1920s and again in the 1940s were administrative in nature and based upon extensive executive authority given to him by the Majless. These were not, as were some others, advisory missions. In

order to have such a power base it was at least required that there be support for reform in some quarters. It is true also, as in the Shuster case that some of this was to forward certain international relations objectives of Iran.

The Millspaugh missions were for awhile able to make great inroads in modernizing the machinery of government and the economy. By encouraging the shah's policy of drastic repression we created a situation where moderate centerist leadership could not continue to develop. That policy led to alienation and indifference on the part of the new middle class.

An illustration of this was that I was repeatedly told by Iranian military officers below the general rank that they were uninterested in what happened to the shah and his government. It is because of the political vacuum created by poor public policy that Khomeini and his faction could come to power.

ABBOTT MILLSPAUGH
Baltimore, Maryland

PROFESSOR KEDDIE REPLIES:

I regard Mr. Millspaugh's letter as a useful supplement to my article rather than a disagreement with it, and I concur with most of his statements. His points regarding disregard for agriculture and overemphasis on urban spending I have made in various articles and in my *Roots of Revolution: An Interpretive History of Modern Iran*, although he may be correct in implying that I should have restated them even in an article stressing other questions. I also agree that U.S. encouragement of the shah's "drastic repression" alienated the new middle class and many army officers.

Mr. Millspaugh's letter points up the importance of historians' doing comparative studies in order to increase our historical knowledge and also to apply it to the present. An Iranian-type overemphasis on cities, consumerism, and certain industries and neglect of agriculture has been widespread in the Third World. They are prevalent particularly in oil-exporting countries and in countries heavily depen-

dent on grants and remittances from the oil exporters, such as North Yemen and Egypt. U.S. aid to governments that are in many ways comparable to the late shah's continues, and such countries are subject to many of the same socioeconomic tensions as were prerevolutionary Iran and Russia.

To date in the United States the comparative historical study of society has been carried out mainly by sociologists like Moore, Wallerstein, and Skocpol, and by a few anthropologists like Wolf, with the Tillys being thus far probably the only prominent American historians in the comparative field of social history. Historians should have a great deal to contribute to the comparative study of revolutions, social movements, and other subjects. Mr. Millspaugh's letter suggests some of the comparative dimensions going beyond the geographical limits of the Middle East which may be found in just one such movement, the Iranian Revolution of 1978-79.

NIKKI KEDDIE
University of California,
Los Angeles

REVIEW ESSAYS

TO THE EDITOR:

I read Jon Jacobson's *Review Essay*, "Is There a New International History of the 1920s?" (*AHR*, 88 [1983]: 617-45) almost immediately after reading John Braeman's "The New Left and American Foreign Policy during the Age of Normalcy: A Re-examination" in the Spring 1983 *Business History Review*. The two articles are companion pieces in dealing with the post-World War I decade. Clearly, to understand that decade historians must look to international rather than purely national considerations.

Jacobson points out that what "constitutes international history has been conceived anew to include the role of private and central banking in international politics, the politics and diplomacy of financial crisis. . . ." He recognizes that international history comprises more than mere political events, yet his article is conventional in that he comes back in each clause of his introduction to "international politics," "politics and diplomacy," "national rivalry," "the influence of multinational cartels on international political stability." But history is more than past politics. Many students of international history are concerned with the 1920s not simply in the political dimension.

The work of numerous economic and business historians has been shortchanged in this article. There is no mention for example of D. H. Aldcroft, *From Versailles to Wall Street, 1919-1929* (Berkeley and Los Angeles, 1977) or of the fascinating work of

Charles Kindleberger, *The World in Depression* (Berkeley and Los Angeles, 1973), which deals at length with the 1920s, the title notwithstanding. Likewise, my work on multinational enterprise in the 1920s, in *The Maturing of Multinational Enterprise: American Business Abroad from 1914 to 1970* (Cambridge, Mass., 1974) is omitted. So, also, for example, is Paul Bairoch's *The Economic Development of the Third World* (Berkeley and Los Angeles, 1975), which contains new figures that provide important insights for students of international history. In fact, the "international history" that Jacobson discusses is conventional in another manner (at least in Jacobson's commentary if not in the monographs themselves) in that Jacobson deals exclusively with the history of Western Europe (with a few asides on the Soviet Union) and with the United States. Economic and business historians have, by contrast, been far more "international." I could go on at length: there is important new work on British and Continental European multinational enterprise in the 1920s. The four volumes of Swedish Match Company history are examples of truly international history. Materials on "multinational cartels" are now being woven into discussions on the nature of international business. These studies use rich data from business archives. The business actor on the international stage in the 1920s is becoming better understood. While some of this work is helpful in explaining why there was such a short period between World War I and World War II, all of it is important in showing how economically integrated the world economy was and how the ramifications of domestic events could spread so quickly over frontiers. Indeed, there is far more that is new on the international history of the 1920s than Jacobson's otherwise excellent article suggests.

MIRA WILKINS
Florida International
University

PROFESSOR JACOBSON REPLIES:

Mira Wilkins demarcates political from economic and business history and then labels the first of these "conventional" and the second two "far more 'international.'" This does not seem to me to be a particularly valid approach to the understanding of the present state of scholarship on the international history of the 1920s. Neither diplomatic nor business history is ipso facto more or less conventional or international. Examples of methodologically advanced and broadly conceived works can be found in each. More significantly, to separate political from economic history is an approach which has been rendered increasingly obsolete by recent scholarship on the decade. Indeed, as I stated in my essay, one of the more promising features of that work is the

utilization of more integrative analyses which incorporate topics and join discourses which had previously been isolated from each other.

"Clearly," as Wilkins states, "to understand the decade historians must look to international rather than purely national conditions." The long crisis of the international world political economy extending from World War I through the Great Depression to World War II has made it difficult for any serious scholar to assert the contrary for decades. And surely the role of international banking and multinational corporations and cartels in the increasingly integrated international economy of the 1920s cannot be denied. As I affirmed in my essay, it is the definition of the parallel, and sometimes interrelated development of national, international, and transnational (or multinational) institutions and policies which is the most challenging task of any synthetic history of the decade.

JON JACOBSON
University of California,
Irvine

REVIEWS OF BOOKS

TO THE EDITOR:

Bernard Lewis's book, *The Muslim Discovery of Europe* (1982), has not received fair treatment in the review by Richard Bulliet (*AHR*, 88 [1983]: 439-40). Bulliet briefly acknowledges that this study is an "impressive and useful contribution," a judgment that comes as no surprise, for Lewis is the doyen of English-speaking Middle East historians. But then the reviewer launches into a sustained attack, accusing the author of bias against Islam. The tenor of this study, he argues, is "derisive and condescending toward Muslims to such an extent that the book's analytical value is seriously undermined." This is a powerful charge which, if it is to stick, must be proven.

But there is no proof. Rather, Bulliet, like all those others who make it a practice to defame Lewis, relies on the attribution of malicious intent. Lewis's scholarly objectives are once again subjected to vicious interpretations; the reviewer presumes that Lewis wishes to denigrate Muslims and finds evidence wherever he can, reading dark meanings into even the most innocuous facts. For reasons of space, two examples must suffice to demonstrate the thrust of the whole review; the reader can then judge for himself.

First, Bulliet objects to the title of the book. He argues that *The Muslim Discovery of Europe* makes Muslims look bad: "Why suggest a comparison between the explosion of knowledge and curiosity in Europe and a tepid lack of interest in the lands of Islam if not to show the latter to be deficient?" If so

manifestly neutral a phrase as *The Muslim Discovery of Europe* conveys Muslim deficiency, no title is safe. Can Professor Bulliet suggest an alternative title which would not be susceptible to his criticism ("Glimpses of the Northern Barbarians")? Indeed, the title of Bulliet's own books could be subjected to similar aspersions: thus, *The Patricians of Nishapur* could be understood as fomenting class differences, *Kicked to Death by a Camel* as deriding Arab customs, and *The Tomb of the Twelfth Imam* as ridiculing the Islamic religion.

Second, Lewis observes that Muslims refrained from learning European languages, leaving this domain to their non-Muslim subjects. He then writes of the few exceptions: "By the second quarter of the nineteenth century, the number of [Muslims] able to read a European language was still remarkably small, and many of them were converts or sons or grandsons of converts from Christianity or Judaism to Islam." The point is clear: so much did non-Muslims dominate this sphere of activity that even those few Muslims who did know European languages had a non-Muslim background. Bulliet, however, draws a nasty conclusion from this: for him, Lewis's "implication is that Christian and Jewish mental vigor can persist genetically for some time against Muslim torpor." This not only misrepresents what Lewis says, but—and here I am admittedly speculating—this misrepresentation appears intentional.

To my mind, Professor Bulliet is perhaps the outstanding younger American historian of the Middle East. The originality of his mind and the quality of his writings have assured him a brilliant career; why then, does he join those seeking to establish a reputation through political attacks on Bernard Lewis?

DANIEL PIPES
Department of State

PROFESSOR LEWIS REPLIES:

Please forgive me for writing somewhat belatedly—after an absence of several months abroad—to comment on Mr. Bulliet's attempt to review my book, entitled *The Muslim Discovery of Europe*. Mr. Bulliet's remarks are an exercise in a kind of reviewing which has become increasingly common of late. The method, briefly, is not to review the book but the author; not his scholarship as set forth in the book and its documentation, but the motives which the reviewer, with neither written evidence nor personal acquaintance, chooses to impute to him. Its aim is neither to inform nor to correct but to discredit.

It is customary for an offended author to refer the reader to his book. Mr. Bulliet fortunately makes it unnecessary to impose any such burden.

His handling of the title is sufficient to characterize his method. "Why" he asks, "suggest a comparison between an explosion of knowledge and curiosity in Europe and a tepid lack of interest in the lands of Islam if not to show the latter to be deficient?" An interesting question, which might be applied to Mr. Bulliet's most serious contribution to Islamic studies, a book entitled *The Camel and the Wheel*. Mr. Bulliet is no doubt familiar with the view that the mere mention of a camel, particularly in so prominent a context as a book title, is clear evidence of deep-rooted hostility to the Arabs and of a desire to play on negative stereotypes. The reference to the wheel in the title of a book mainly devoted to discussing why the Arabs made so little use of it, is obviously a sneer at their technological backwardness.

This is of course nonsense—but no more so than Mr. Bulliet's own exegesis of *The Muslim Discovery of Europe*. The Humpty-Dumpty method of interpretation is also used on some other points in the book. One example may suffice. I had observed that educated Muslims until a late date were unwilling to learn European languages. Mr. Bulliet counters indignantly by pointing to their study of Arabic, Persian, and Turkish which are very difficult languages. Mr. Bulliet thus blurs the distinction between willing and able—whether through his own unwillingness or inability to see it, I am unable to say. The same question arises concerning some of Mr. Bulliet's other interpretations, such as the "implication" which he claims to find—citing p. 301—"that the Islamic religion propagates [*sic*] primarily by warfare." There is no such implication, neither on page 301 nor on any other, and Mr. Bulliet might credit me with the rudimentary knowledge of Islamic history required to know that such an "implication" is false.

BERNARD LEWIS
Princeton University

TO THE EDITOR:

As a book reviewer for the *American Historical Review* for the past fifteen years, I find the notice of my recent book, *Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI* (*AHR*, 88 [1983]: 670) a clear departure from your guidelines. The reviewer's analysis is inaccurate, and his evaluation is biased. He also knows that records giving deeper insight into the rebels' motives do not exist. The reviewer may have enjoyed writing this polemic, but readers will have to consult other journals for a responsible review of *Rebellion and Riot*.

BARRETT L. BEER
Kent State University

PROFESSOR HOAK REPLIES:

In a form-letter sent to this prospective reviewer in August 1982, the Editors of the *AHR* enumerated

six guidelines. Five comprehended mechanical and stylistic points ("... Leave the top half of the first page blank ...," etc.). The remaining one spelled out the only substantive charge ever conveyed to me: "The review should give a clear statement of the book's contents and a critical assessment of its contributions to knowledge in its field."

In my review, in four paragraphs of roughly equal length (about 125 words each), I tried successively to (1) suggest the nature and significance of the period and subject in question; (2) reveal the purpose and scope of the book (I quoted the author's self-stated aims); (3) identify the relevant topics treated and indicate the proportion of the whole given over to each; (4) provide, in light of recent research on the subject, the "critical assessment" demanded by the Editors. Stylistically, I found it natural to weave some of the assessment into the second and third paragraphs as well.

Did you, sir, find my review "a clear departure from your guidelines"?

In any case, where are the inaccuracies Professor Beer alleges to have found? He specified none.

I possess no knowledge of the nonexistence of records of the type to which Beer refers. It was not Beer's sources, but his outmoded methodology that failed him: he was not able adequately to describe the context of the rebels' actions. Why he was not able to do so remains puzzling, since for Kett's Rebellion, at least, he had before him the contextual example provided by Diarmaid MacCulloch in 1979—all of which I pointed out in my review. If this be polemic. . . .

Bias of some sort stamps the work of every historian. Professor Beer's apparently predisposed him to appreciate only those reviews which would favor his book. I know my own biases, and they are clearly not the ones imagined by Beer, since he failed to recognize how much I value first-class historical scholarship.

DALE HOAK
College of William and Mary

TO THE EDITOR:

Despite (or because of) being both a former history major and a member of the Book-of-the-Month Club, I had no intention of buying Leonard Mosley's *Marshall: Hero for Our Times*. After reading Warren I. Cohen's highly entertaining review (*AHR*, 88 [1983]: 776-77), however, I have a problem. Whenever I pass a bookstore, I feel an almost overwhelming desire to know what secret the "Hero's" first wife told him on their wedding night. Dr. Cohen, help me before I buy this book!

BURDEN S. LUNDGREN
Norfolk, Virginia

Although offered 700 words (in conformity with the *Review's* rules), the reviewer refused our invitation to respond to this communication.

THE EDITORS

TO THE EDITOR:

In your October issue (*AHR*, 88 [1983]: 1077–78), Paul C. Nagel dismisses my book *Both Sides of the Ocean: A Biography of Henry Adams, His First Life, 1838–1862*. He alleges that I have not “used consistently the wealth of relevant manuscript and secondary sources that has appeared” since I began my work as biographer. As a main example, he reports that I “evidently” have not used the diary of Charles Francis Adams, Henry’s father.

The *first page* of my narrative uses that diary as the source of my account of Henry’s birth, says the source used is that “diary,” and dates the entries.

If my narrative drew explicitly upon the father’s diary only once, twice, or even five times, I could understand your reviewer’s failing to notice my using it. But the narrative, by my count, draws explicitly on the diary in twenty-five connections. Of these chances to see that I do use the diary, Nagel missed all twenty-five. I would judge that so spectacular an oversight raises a doubt about his competence and conscientiousness as a reviewer of books.

Your reviewer alleges that I have “disregarded” the works of other writers, most notably the biography of Henry Adams by Ernest Samuels.

I specify that the “writings” of Samuels (which extend beyond his biography) have “aided me with source materials, information, and ideas concerning Adams” (p. 14). (Since Nagel neglects to say so, let me mention that I am a *research* biographer and my book is based, as far as possible, on *primary* materials, including early writings by Adams—both published and in manuscript—new to other inquirers.)

Nagel further alleges “exaggerations.” “The greatest blunder is when Charles Francis Adams . . . is misrepresented. Chalfant makes him into a weak bumbler.”

My book takes a middle position concerning C. F. Adams. It deals with him mainly in two connections: his service as congressman and his first efforts as minister to England, ending with the resolution of the *Trent Affair*. I describe him, in the former capacity, as “a paragon of competence” (p. 207). I describe him, in the latter capacity, as making a good first impression in England (p. 256), as unsuccessful in a negotiation relating to the Treaty of Paris (p. 298), and as gloomy during the *Trent Affair* (pp. 332, 345). Opposing the view of his biographer (Martin Duberman) that he remained “a passive and impotent spectator” throughout that great crisis, I show that he had “a decided effect on events in Washington” (p. 444 n. 17). I rate him less capable than the Adams presidents and Henry, explain that

he was “not a leader . . . but an exceptionally capable follower” (p. 137), and show him to have become Seward’s follower—also as less effective in the crisis than Seward or Weed (see relevant index entries under Seward, Weed, and C. F. Adams). So where is the exaggeration, or the “weak bumbler”?

Your reviewer alleges, too, that the view of Henry Adams set forth in my book was originated by women who knew him, “blindly adored” him, and attempted to “foster a reverent admiration for his memory.” Nagel identifies Louisa Hooper Thoron and Aileen Tone as blind adorers; he says my book is “devoted to the outlook of these women”; and he claims that—according to me—Mrs. Thoron and Miss Tone assisted in the preparation of my “early drafts.”

The view of Adams presented in my book is my own. I nowhere say that either woman helped prepare my drafts or altered my basic understanding of my subject—for the good reasons that I prepared my drafts myself and that I formed my basic understanding of Adams independently, by studying the already considerable sources, in 1941–52, before I knew those estimable persons. It is nothing against me that the idea of Adams’s *character* that I formed by this means coincided with the idea formed by Mrs. Thoron and Miss Tone through personal acquaintance, that I asked them to *read* my early drafts, and that they supplied valuable information and suggestions.

What matters most is that, right or wrong, my ideas are mine. A myth-making reviewer should not be allowed to award them high-handedly to other people in print in your journal, without your readers being protected against delusion.

EDWARD CHALFANT
Hofstra University

DR. NAGEL REPLIES:

Perhaps the most helpful response to Professor Chalfant’s communication is for me to urge that interested persons read his book, and particularly its introduction and notes. Here will be found frequent mention of Chalfant’s rewarding association with Mrs. Thoron and Miss Tone. Here, too, is his only reference to Ernest Samuels, which is to group him with writers from generations long gone—(“... Charles Francis Adams, Jr., Brooks Adams, John Franklin Jameson, William Roscoe Thayer, Worthington Chauncey Ford, Ephraim Douglass Adams, Ward Thoron, and Ernest Samuels . . .” p. 14). The most recently published book in Chalfant’s bibliographical listing is Martin Duberman, *Charles Francis Adams*, which appeared in 1961. C. F. Adams’s diary is occasionally mentioned in Chalfant’s “additional” or “lesser” citations, an approach to this monumental source which, under the circumstances, left me uneasy about its serious use.

Those who do read this biography, which discovers a "preternatural" Henry Adams, will ponder Chalfant's closing assertion: "Despite all I say above about imagining, my experience has *not* been one of inventing a story. Rather it has been that of being given one" (p. 392).

PAUL C. NAGEL
Virginia Historical Society

COMMUNICATIONS

TO THE EDITOR:

My attention has been drawn to letters from Professors Conrad Russell and Richard Greaves which appear to concern myself and my writings (*AHR*, 88 [1983]: 1140–41). The Irishman who asked if it was a private fight or whether anyone could join in would have had a valid point if the quarrel had concerned his own property. This is the invidious position I find myself in, my property in this case being the view I take of religion in Elizabethan and Jacobean England. I even wonder whether it is safe to cross the Atlantic, which I propose to do, for the first time in my life, later this year. While I brace myself for the experience, I shall be grateful if Professor Greaves will tell me where and when I have "tried to cloak revisionism in the Union Jack." The allegation is almost actionable, given my lamentably antipatriotic views on sundry public questions. The still greater irony of the spectacle of Bertrand Russell's son wrapping himself in the Union Jack is perhaps lost on Professor Greaves. I discriminate between good and less than good scholarship, truth and error, not between historians who have had the fortune or misfortune to have been born under different flags. Elsewhere in his letter, Professor Greaves misrepresents me in the matter of distinguishing between "Puritans" and "Anglicans." The point at issue is one of categories, and I am not sure that he has understood it. Evidently Professor Greaves addresses himself not to *The Elizabethan Puritan Movement* (1967 rather than "1966") but to my more recent *Religion of Protestants* (1982) and possibly to a review of his own *Society and Religion in Elizabethan England* which appeared in the *Times Literary Supplement*. But if he refers to the Preface to my *Godly People* (1983) (which reprints the articles and essays of twenty-five years without violence to my present views) he will see that I suggest that "revisionism" will have gone too far if it forgets the existence of militant Elizabethan Puritanism. And in my contribution to *Before the English Civil War*, ed. Howard Tomlinson (1984), I say that "in spite of the doubts currently expressed by some historians, Elizabethan Puritanism is properly described as a movement." See also my (British) Historical Association pamphlet *English Puritanism* (1983). Of course it was absurd to dub me an "extreme revisionist." As a matter of fact, I would not call myself a revisionist of

any kind. (But "Puritans" did not call themselves Puritans, and "Anglicans" were not called Anglicans by anybody.) Among those who know me and understand my work I have a reputation for no great profundity but for a certain noncommittal subtlety which is not well described as extreme anything. In spite of Professor Greaves's strictures, I look forward to my first visit to North America.

PATRICK COLLINSON
University of Kent

PROFESSOR GREAVES REPLIES:

The heart of this matter involves the meaning and use of the terms "Anglican" and "Puritan." Collinson and I agree that "Anglican" is an anachronistic term and "Puritan" originally a pejorative one. However, I find the alternatives to "Anglican" (e.g. "Conformist" or "Formalist") less satisfactory than Collinson does. We agree that Elizabethan Protestantism cannot be "neatly divided into two well-defined and clearly labelled parties," a position neither of us has ever espoused, though Collinson has erroneously suggested that I have. In *The Religion of Protestants* (1982) Collinson observes that it is a "damaging mistake" to write the history of the Church of England in the prerevolutionary period "in the anachronistically dichotomous terms of an Anglicanism not yet conceived and an alien puritanism not yet clearly disowned" (p. ix). On this too we concur. Collinson, however, has wrongfully accused "historians, especially in America," of regarding Puritan views as "alien to the Protestantism of the Church of England" (p. viii). I know of no American specialist who holds such a view, and therefore of no justification for such an unfounded nationalistic indictment.

In his 1982 book, Collinson refers several times to "puritans and protestants" (pp. 189, 190, 196, 275). Standard English usage (apples and oranges, not apples and fruit) indicates that on these occasions Collinson does not regard "Puritans" as Protestants. Is it a matter of subtlety that sometimes the "Puritans" are Protestants, sometimes not? The readers of the *Review* will also have to judge for themselves whether Collinson is engaging in "noncommittal subtlety" when he asserts that "Puritans" were not, in his words, "real, identifiable people," but yet that there was such a thing as "militant Elizabethan Puritanism." Is Collinson suggesting that Thomas Cartwright and Walter Travers and William Perkins and Job Throckmorton were not real people, or simply that there was a movement to which no one belonged? A *non sequitur* is not a subtlety. It seems to me that anyone who denies that "Puritans" were "real people," or whose language sometimes implies that "Puritans" were not Protestants, has advocated an extreme point of view. If Collinson is now recanting these assertions, then the differences in

our respective interpretations are slight indeed. Either way, American scholars—none more than I—warmly welcome his visit.

RICHARD GREAVES
Florida State University

ERRATA

In my article with Stanley Engerman and Manuel Moreno Fraginals, "The Level and Structure of

Slave Prices on Cuban Plantations in the Mid-Nineteenth Century" (*AHR*, 88 [1983]: 1201–18), a fundamental error appeared in the conclusion (p. 1218). Line 10 should correctly read as follows: "Rural plantation regimes generally valued prime-age healthy, unskilled women 5 to 20 percent below [instead of "above"] men."

HERBERT S. KLEIN
Columbia University

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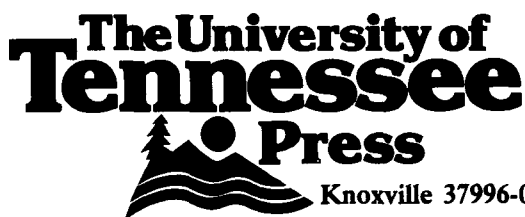
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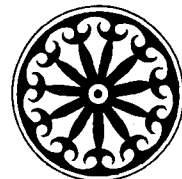
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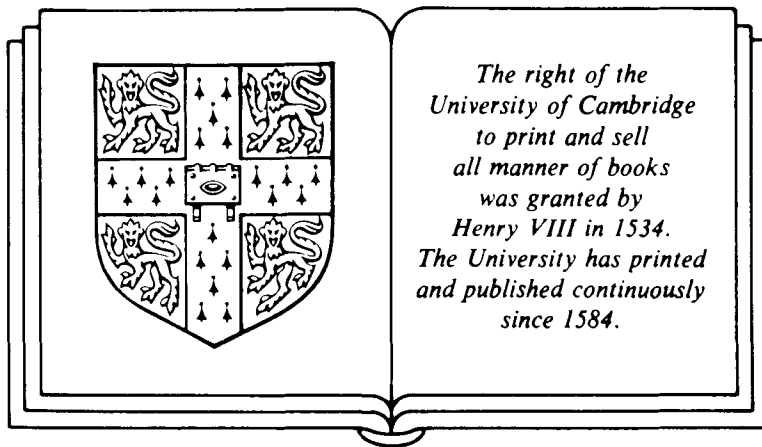
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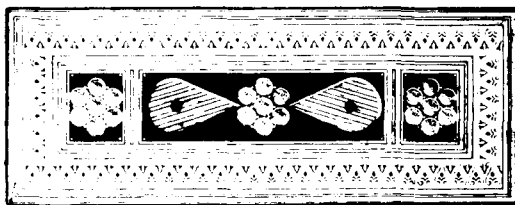
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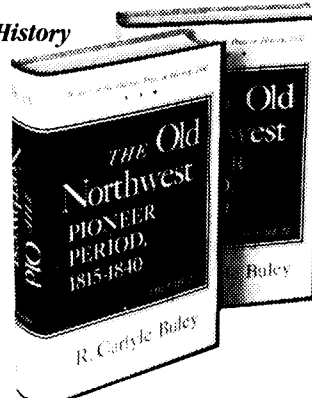
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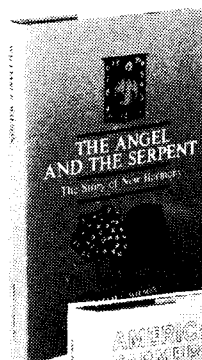
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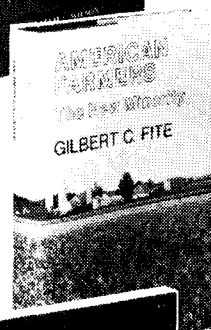
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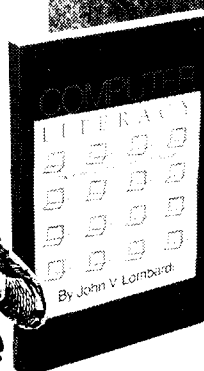
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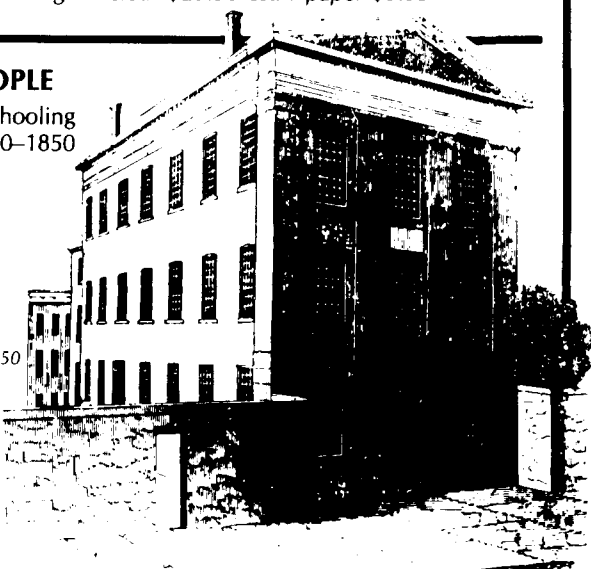
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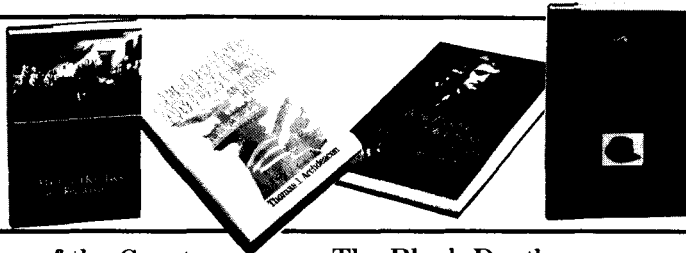
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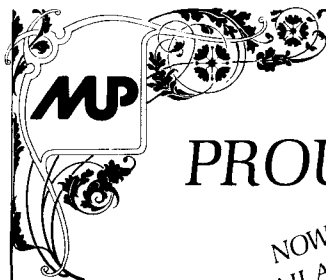
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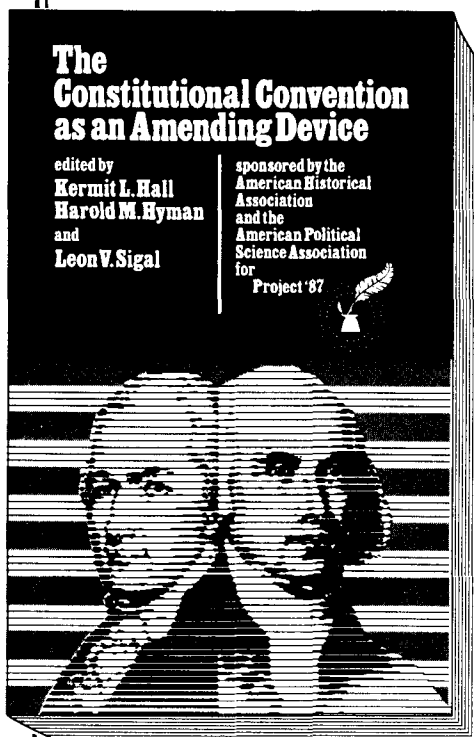
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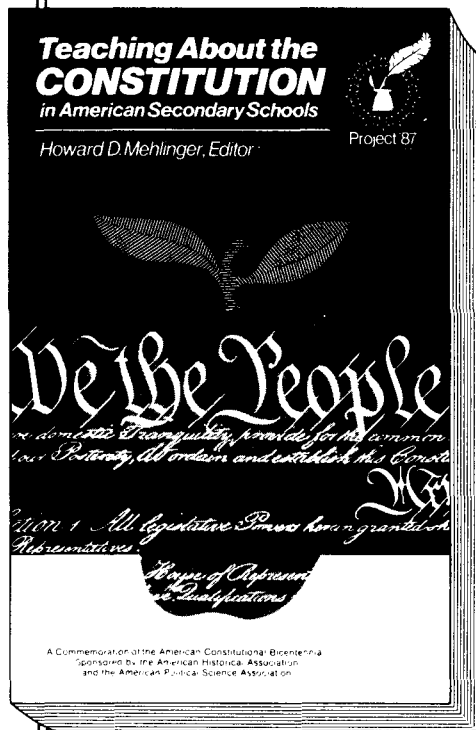
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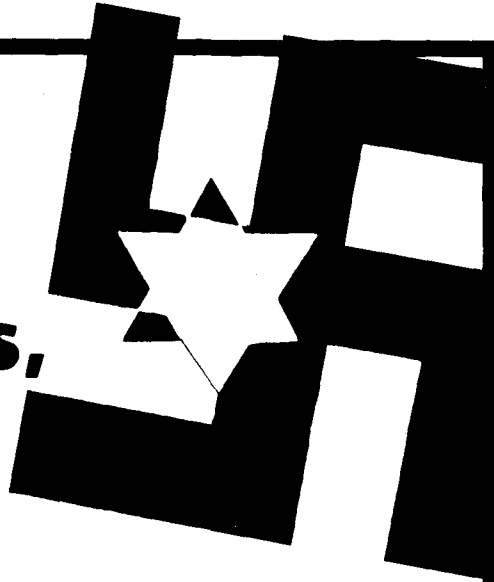
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